RUHR

ECONOMIC PAPERS

Giacomo Corneo
Matthias Keese
Carsten Schröder

The Effect of Saving Subsidies on Household Saving

Evidence from Germany

Ruhr Graduate School in Economics

UNIVERSITÄT DUISBURG ESSEN

#170

Imprint

Ruhr Economic Papers

Published by

Ruhr-Universität Bochum (RUB), Department of Economics
Universitätsstr. 150, 44801 Bochum, Germany

Technische Universität Dortmund, Department of Economic and Social Sciences
Vogelpothsweg 87, 44227 Dortmund, Germany

Universität Duisburg-Essen, Department of Economics
Universitätsstr. 12, 45117 Essen, Germany

Rheinisch-Westfälisches Institut für Wirtschaftsforschung (RWI)
Hohenzollernstr. 1-3, 45128 Essen, Germany

Editors

Prof. Dr. Thomas K. Bauer
RUB, Department of Economics, Empirical Economics
Phone: +49 (0) 234/3 22 83 41, e-mail: thomas.bauer@rub.de

Prof. Dr. Wolfgang Leininger
Technische Universität Dortmund, Department of Economic and Social Sciences
Economics – Microeconomics
Phone: +49 (0) 231/7 55-3297, email: W.Leininger@wiso.uni-dortmund.de

Prof. Dr. Volker Clausen
University of Duisburg-Essen, Department of Economics
International Economics
Phone: +49 (0) 201/1 83-3655, e-mail: vclausen@vwl.uni-due.de

Prof. Dr. Christoph M. Schmidt
RWI, Phone: +49 (0) 201/81 49-227, e-mail: christoph.schmidt@rwi-essen.de

Editorial Office

Joachim Schmidt
RWI, Phone: +49 (0) 201/81 49-292, e-mail: joachim.schmidt@rwi-essen.de

Ruhr Economic Papers #170

Responsible Editor: Volker Clausen

ISSN 1864-4872 (online) – ISBN 978-3-86788-190-6

Ruhr Economic Papers #170

Giacomo Corneo, Matthias Keese, and Carsten Schröder

The Effect of Saving Subsidies on Household Saving

Evidence from Germany

Ruhr Graduate School in Economics

UNIVERSITÄT DUISBURG ESSEN

Bibliografische Informationen der Deutschen Nationalbibliothek

Die Deutsche Bibliothek verzeichnet diese Publikation in der deutschen Nationalbibliografie; detaillierte bibliografische Daten sind im Internet über: *http//dnb.ddb.de* abrufbar.

ISSN 1864-4872 (online)
ISBN 978-3-86788-190-6

Giacomo Corneo, Matthias Keese, and Carsten Schröder[1]

The Effect of Saving Subsidies on Household Saving – Evidence from Germany

Abstract

Since 2002 the German government seeks to stimulate private retirement savings by means of special allowances and tax exemptions – the so-called Riester scheme. We apply matching and panel regression techniques to assess the impact of the Riester scheme on households' propensities to save in a natural experiment framework. Estimation results from both the German Socio-Economic Panel and the SAVE Study indicate that private saving was hardly affected by the Riester scheme.

JEL Classification: D12, D14, H24, H31, I38

Keywords: Household saving; saving incentives; retirement; Riester scheme; Coarsened Exact Matching

February 2010/May 2011

1 *Giacomo Corneo, Free University of Berlin, Department of Economics, Chair of Public Economics; Matthias Keese, Ruhr Graduate School in Economics (RGS Econ) and University of Duisburg-Essen; Carsten Schröder, Christian-Albrechts-University of Kiel, Department of Economics, Chair of Public Economics, Social Policy and Health Economics.* *– We owe special thanks to Reinhold Schnabel. Furthermore, we received valuable comments at the RGS Workshop at the University of Duisburg-Essen and the Public Economics Colloquium at the Technical University of Dortmund.* *– All correspondence to Matthias Keese, Ruhr Graduate School in Economics, c/o University of Duisburg-Essen, Chair of Public Economics (Prof. Dr. Reinhold Schnabel), 45117 Essen, Germany, E-Mail: matthias.keese@uni-due.de.*

1 Introduction

A major ingredient of governmental responses to demographic changes eroding the financial basis of pay-as-you-go (PAYG) pension systems has been to favor household saving for retirement purposes. Currently, certified financial instruments for retirement saving are promoted by means of tax deductions and subsidies in several countries.[1] A crucial issue about such government-sponsored retirement plans is whether households finance their contributions with genuinely new saving – that would not have been done in the absence of those incentives – or with reductions in other assets, including increased borrowing. While new savings add to national wealth and raise future national income, a mere reallocation of financial assets has, if any, ambiguous effects on future national income. Hence, the evaluation of tax-favored retirement plans hinges upon their impact on households' saving behavior. Since 2002 also the German government supports private retirement saving plans by means of a saving incentive program called the *Riester scheme*. Meanwhile, generous incentives and pessimistic expectations about future pension benefits from the PAYG system have led a substantial fraction of the German population in working age to participate in the Riester scheme. This paper presents estimates of the effect of the Riester scheme on the saving propensities of German households.

The extent to which tax incentives and subsidies raise private saving is still an unresolved issue. For the eligible households, standard theory does not offer an unambiguous prediction because of countervailing income and substitution effects from a higher net return on saving. Further insights are offered by behavioral economics. Subsidized private pension schemes may increase households' savings if those schemes include penalties from early withdrawals that act as a valuable self-control device for savers. However, behavioral approaches may also predict that subsidized schemes reduce private saving. To the extent that households follow the rule of saving enough to replace a fixed percentage of their income in retirement, a higher net return on saving reduces the amount of saving necessary for that replacement. Furthermore, the savings of households not eligible for the subsidy may be affected. If the subsidy is financed by increased taxes on non-eligible households or by reducing the transfers that they receive, the saving by non-eligible households is likely to diminish.

Previous empirical research on the effectiveness of saving incentives has dealt overwhelmingly with the US experience (Antolín *et al*., 2004, Annex 2). In the United States, 401(k) has become the main vehicle for retirement saving and much attention has been

[1] See Antolín *et al*. (2004) and Yoo and de Serres (2004) for overviews.

devoted to evaluate its effectiveness. Early influential papers by Engen *et al*. (1994) and Poterba *et al*. (1995) presented results from median regressions and reached quite contradictory conclusions about the substitution between 401(k) assets and other type of savings. Recent papers, employing more sophisticated estimation techniques, have tended to find much heterogeneity in households' responses to 401(k)s and substantial crowding-out effects in the case of high-income households (Benjamin, 2003; Chernozhukov and Hansen, 2004).[2] For Germany, Corneo *et al*. (2009) have evaluated the Riester scheme as a natural experiment which affects the saving propensity of a treatment group relative to a control group. Their findings cast some doubts on the effectiveness of the Riester scheme in terms of mobilization of new savings.[3]

The current paper substantially extends the work presented in Corneo *et al*. (2009) along four main dimensions. First, we consider a broader set of treatment and control groups. In particular, we compare changes in the saving propensities of households eligible for Riester subsidies with the changes in saving propensities of non-eligible households, changes in the savings of households who benefit from high subsidies relative to those who receive low subsidies, and changes in the savings of eligible households having a Riester contract and those who do not. Second, we exploit statistical matching and panel regression techniques to address important issues of self-selection and unobserved heterogeneity. Third, in addition to the German Socio-Economic Panel we use the SAVE dataset, which has been explicitly designed to investigate the saving behavior of private households in Germany. Fourth, we provide not only an evaluation in a pre- *vs*. post-reform perspective but also an assessment of the impact of the so-called *Riester steps*, namely the stepwise increase in subsidies and required saving amounts over time.

In order to check the robustness of our empirical findings, we rely on three estimation methods. First, we use random-effects tobit panel models to regress saving rates before and after the reform on a dummy distinguishing treated and non-treated subjects after the reform, a post-reform dummy, and a set of socio-economic characteristics. Size and sign of the marginal effects of the *treatment* dummies serve as indicators of a stimulating effect of the Riester scheme in the various approaches. Second, we take first differences of the savings ratio and other explanatory variables and run OLS regressions in first differences. Thereby, treatments effects are identified by sign and size of the *treatment* coefficient. Third, we

[2] See also Duflo *et al*. (2007) who have evaluated the "saver's credit", a US federal program to encourage retirement savings, finding modest effects.
[3] Börsch-Supan *et al*. (2008b) and Pfarr and Schneider (2009) have investigated the determinants of participation in the Riester scheme. The uptake of Riester contracts offers circumstantial evidence of displacement effects.

provide estimates for a subsample where treated and control subjects share the same socio-economic and demographic characteristics. We identify such statistical twins using a matching algorithm recently proposed by Iacus *et al*. (2008). Their algorithm ensures that treated and control units are approximately balanced on the matching variables. With the matched observations at hand, it is then possible to infer the average treatment effect on the treated.

Even though we apply a wide range of methods and specifications, we come to the unambiguous conclusion that in Germany household saving hardly responded to the introduction of that saving incentive program. Participation in the Riester scheme seems to largely substitute for other forms of saving. More specifically, we find insignificant treatment effects in all first-differences regressions and most random-effects specifications. These outcomes are reconfirmed by the matching results of all approaches and year combinations. That we find significantly positive mobilization effects on private savings in some random-effects specifications can be attributed to self-selection and endogeneity bias. Moreover, our main implication is similar for evaluations of the reform itself (comparing savings before and after the introduction of the reform with SOEP data) and of the progression of the reform steps (comparing savings at different points in time during the reform with SAVE data).

The remainder of this paper is organized as follows. Section 2 describes the functioning of the Riester scheme. The econometric modeling is described in Section 3. Our databases, the German Socio Economic Panel and the SAVE Study, are presented in Section 4. In Section 5, we discuss our results and conclude in Section 6.

2 The Riester scheme

The Riester scheme started operating in 2002. Beneficiaries receive allowances (a basic allowance and child allowances), and can lower their income tax liability by means of deductions. A minimum saving effort is requested from the beneficiaries. More precisely, the allowance and the personal saving effort must add up to a specific amount, which is proportional to the individual's income subject to social insurance contributions. The minimum saving amount is defined as a share of the income subject to social insurance contribution of the previous year, including the allowances. This share increased stepwise from one percent in the first year to four percent in 2008. These so-called Riester steps are displayed in Figure 1. Also the level of allowances and the maximal amount of tax deductions

have been increased stepwise since the introduction of the Riester scheme.[4] If the Riester scheme stimulated private savings, its mobilization effect should be visible in a pre- and post-reform comparison as well as along the Riester steps: the higher the required minimum savings amount and the subsidies granted, the higher the household savings.

Figure 1 about here

A large portion of the active population in Germany is eligible for Riester subsidies, estimates going up to 36 million people (Bräuninger, 2005). Basically, all compulsorily insured persons in the German public pension system are eligible for Riester contracts. In addition, public servants, trainees, individuals in the mandatory military or social service, and the recipients of some types of public transfers (e.g., unemployment benefits) may participate. Persons who are not statutorily insured in the mandatory public pension system are usually not eligible. Those persons include several groups of self-employees, marginal employees and students, social welfare recipients, and senior citizens receiving a pension.[5]

The impact of the Riester scheme on national (private plus public) saving also depends on its effect on the public debt. An exact calculation of the fiscal burden from the Riester scheme can only be performed with some delay because the deadline of application for a certain contribution year is two years later. Table 1 provides an overview of the current fiscal costs of the Riester scheme. The non-italic figures show the actual allowances and tax deductions. Assuming a constant relation between allowances and tax deductions as well as a proportional relation of Riester contracts on the one hand and both allowances[6] and tax deductions on the other hand, our extrapolation (italic figures) yields annual direct costs of 2.8 billion euros for 2008, which is about 2.7 percent of public expenditure in that year (see Federal Statistical Office at http://www.destatis.de), and in the following years, depending on how the uptake of Riester contracts develops. In addition, indirect costs for certification, administration, etc. have to be accounted for.

Table 1 about here

[4] Schulze and Jochem (2007) provide a detailed introduction to the German pension system and its recent reforms, including the Riester scheme. The political economy of the Riester reform has recently been analyzed by Kemmerling and Neugart (2009).

[5] Eligibility regulations are very detailed and include a broad range of exemptions. Furthermore, there have been some adjustments in the regulations. We base our analysis on the legal framework as illustrated by the Federal Ministry of Labour and Social Affairs (2006).

[6] The child allowance is notably higher for children born in 2008 and later (Federal Ministry of Labour and Social Affairs, 2006). Therefore, child allowances as a share of overall costs may increase in the next years.

3 Empirical strategy

3.1 Definition of treatment approaches

We scrutinize the impact of the Riester scheme on households' saving propensities by means of a treatment analysis. In order to assess the causal effect of the reform we compare pre- and post-reform propensities to save for two groups, a treatment group and a control group. Thereby, the SOEP offers numerous possibilities for econometric analyses of the Riester scheme given the length of this panel and its high number of surveyed households. Since people might have anticipated the Riester reform and correspondingly adjusted their pre-reform savings, we use the year 2000 and not 2001 as the pre-reform period. To cope with the possibility that people adjusted their savings with some delay and to address the stepwise increase in subsidies and required savings, we evaluate various post-reform years, from 2004 to 2007.

Various definitions of the treatment and the control group are considered. The characteristics of the treatment and the control group for all five approaches are summarized in Table 2. Underlying our econometric analysis always is a balanced working sample complying, in the pre- and in the post-reform period, with the characteristics outlined in Table 2. Household units having different characteristics are discarded. We provide a detailed description of our proceedings in the appendix.

Maybe the most straightforward possibility (*Approach 1*) is to assign households to the treatment group if all adult household members are eligible for a Riester contract, and non-eligible households to the control group. The strength of Approach 1 is that it does not impose strong restrictions on the characteristics of households entering the working sample so that the working sample size is large. A weakness of Approach 1 is that treated and control households exhibit quite different personal characteristics.

A second procedure focuses on low-income households, a target group of the Riester scheme. Low-income households can benefit from particularly high subsidy ratios. In *Approach 2*, households eligible for Riester with an equivalent household income below the average in the respective period are assigned to the treatment group. Households not eligible for Riester and at an income level below the mean are assigned to the control group. Approach 2 allows us to evaluate the impact of the Riester scheme on a target group of the reform, namely low-income households. Moreover, treated and control households' economic situation, as determined by household income, is similar. So, both should have a similar propensity to save. By nature, a backdrop of Approach 2 compared to Approach 1 is a substantial reduction of sample size.

A third option is based on the fact that Riester subsidies are higher for households with more children, potentially creating an extra incentive to save (*Approach 3*). The basic idea is that the additional child allowance for the second child induces an additional saving incentive to the *treated* households which potentially benefit from a particularly high saving subsidy. Particularly, married couples with two children in the post-period period form the treatment group. Married couples with one child are assigned to the control group. At a gross income exceeding a particular threshold, benefits from tax exemptions resulting from a Riester scheme may top the usual subsidy paid as allowances. This threshold is sensitive to household composition. Accordingly, it is not guaranteed that households with more children benefit from a higher subsidy (in form of tax allowances) over the entire income range. For this reason, the working sample is restricted to households with an income below average. This latter restriction makes sure that the additional child allowance does indeed increase the subsidy ratio of the households under investigation.

In *Approach 4*, we select all households potentially eligible for a Riester contract in the post-reform period. We classify them conditioning upon whether the household head has signed a contract or not. As participating in a Riester contract is a choice, the group-classification criterion is not exogenous. To avoid misunderstandings, instead of referring to *treated* and *controls*, we may better call them *subscribers* – eligible household heads having signed a Riester contract – and eligible household heads who have not signed such a contract in the period under consideration.

Finally, *Approach 5* is similar to Approach 4 but regards households as been *treated* if any adult household member has signed a Riester contract.

Table 2 about here

When commenting upon our findings, we shall concentrate on the year combinations 2000-2004 and 2000-2005 for Approach 1-3 as well as 2000-2004, 2000-2006, and 2000-2007 (since the uptake of Riester contracts was only surveyed in those years) for the fourth and fifth approach when using the SOEP data. The 2000-2004 comparison is our preferred one because 2005-2007 savings are possibly affected by other factors as well, such as the introduction of so-called Rürup pensions in 2005[7].

[7] Rürup pensions are subsidized private retirement saving contracts especially targeting people that are not mandatorily insured in the German pension scheme, e.g., the self-employed. Contributions are tax-deductible, and the accumulated capital is repaid as a monthly annuity. For details, see, e.g., the homepage of the Federal

We complement these findings with regression results based on observations from the SAVE Study as surveyed in 2005-2008. The relevant observation points for both datasets are displayed in Figure 1.

3.2 Econometric challenges

Our analysis might suffer from self-selection bias. For instance, the underlying working samples could have a different saving behavior compared to the household units being excluded. Then, the saving behavior of the considered households would not be representative for the entire population. For the approaches 1-3, we can rule out that households self-select into a different group between the two points of observation or that they self-select into a status that is not captured by our treatment or control group definitions. In these approaches, *treatment* is linked to Riester eligibility (and, therefore, to employment and marital status) and to household composition. It seems very unlikely that fundamental household decisions such as occupation, marriage, or birth of children are driven by considerations related to the Riester scheme. On the contrary, self-selection is virulent in Approach 4 and Approach 5 since the conclusion of a contract is voluntary. However, we apply different strategies to deal with this issue (described below).

Based on the aforementioned sample classifications, we evaluate the impact of the Riester scheme on household saving ratios, i.e., household savings divided by household net income. Since our dependent variable is censored from below (respondents participating in both datasets do not report negative saving amounts) and as our data exhibit a panel structure, we have chosen a random-effects tobit model, building on the form

$$(1) \qquad s^{*}_{i,t} = \alpha + \mu \cdot N_{i.\,t} + \beta \cdot R_{i\,t} + \delta \cdot x_{i\,t} + v_i + \varepsilon_{i,t}$$

In Eq. (1), i identifies a specific household, t denotes the observation period with $t=1$ denoting the period before the reform. Although we consider different post-reform years, with little abuse of notation, $t=2$ always indicates the post-reform period. The coefficient v_i is the random effect, and $\varepsilon_{i,t}$ the error term. The random effect v_i is assumed to be independent and identically distributed according to $N(0,\sigma_v^2)$. The vector of socio-economic characteristics of household i in t is denoted by $\mathbf{x}_{i,t}$. The variable $R_{i,t}$ distinguishes households belonging to the treatment and to the control group. In $t=1$, it is always zero. In the post reform period, for

Ministry of Finance at http://www.bundesfinanzministerium.de/nn_39846/DE/BMF__Startseite/Service/Glossar/B/019__Basisrente.html.

treated households $R_{i,2}=1$ while else it is zero. Hence the corresponding regression coefficient β mirrors the mobilization effect of the Riester scheme. In particular, $\beta>0$ would indicate that the Riester reform has stimulated savings among treated households. N is another dummy variable, taking the value one if the observation refers to a post-reform year; its coefficient captures the evolution of saving ratios between two observation periods. Among the control variables we include dummies for different household types, household income, employment status and age of the household head, and dummy variables on repayments for consumer credit and home loans.

As already mentioned, classification in our fourth and fifth approach is not strictly exogeneous. Saving preferences (e.g., due to risk-aversion or individual discount factors) are likely to be correlated with the conclusion of a Riester contract. We can expect that households with a higher propensity to save are more likely to sign up such a contract. Moreover, the Riester scheme is intended to serve as an entry to private old-age provision. The providers of Riester products have to go after governmental certifications for their products. Thus, we might find numerous households with low experience in financial affairs among Riester savers. In any case, these household-specific effects are potentially unobserved and we do not have a convincing proxy (or, alternatively, a valid instrument variable) at hand to deal with this endogeneity issue. Consequently, we would obtain biased results. Under the assumption that unobserved heterogeneity with respect to saving preferences and experience is time-invariant and that time paths of savings are similar, we can infer the treatment effect by means of a first-difference estimator of the following form,

$$(2) \qquad \Delta s_i = \mu + \beta \cdot R_{i,2} + \delta \cdot \left(x_{i,2}-x_{i,1}\right)' + (\varepsilon_{i,2}-\varepsilon_{i,1})$$

where $\Delta s_i = (s_{i,2} - s_{i,1})$ denotes the difference in i's saving rates between the period after the reform, $t=2$, and before, $t=1$. Similarly, the vector $\mathbf{x}_{i,2} - \mathbf{x}_{i,1}$ stands for inter-temporal changes in the socioeconomic covariates. The time-invariant individual effect v_i cancels out. Again, $\beta>0$ would suggest a stimulation effect from the reform.

3.3 Motivation of matching

If the treatment variable is not independent of the background covariates, regression-based estimates of the treatment effect may depend on modeling choices and regression specifications. Particularly, if the functional relationship between the treatment variable and the background covariates is misspecified, estimates of the *true* treatment effect can be biased.[8] Successful matching breaks the link between the treatment variable and background covariates, produces data similar to a randomized experiment, and eliminates the problem of model/specification dependence. Accordingly, in the third place, we provide estimates of the average treatment effect on the treated, ATT, by using matching methods. We construct a sample such that the distributions of several characteristics are similar in the groups of treated and control units (Iacus *et al*., 2008). For the sample of matched units, the ATT is elicited through a weighted ordinary least square regression of the form

$$(3) \qquad \Delta s_i \cdot \sqrt{w_i} = \mu \cdot \sqrt{w_i} + \beta \cdot R_{i,2} \cdot \sqrt{w_i} + (\varepsilon_{i,2} - \varepsilon_{i,1}) \cdot \sqrt{w_i}$$

where w_i is the matching weight. Table 3 summarizes the matching variables: household net income, age of the household head, and household type. The selection of the matching variables is guided by previous literatures on household saving suggesting that these variables have a prominent effect on the saving behavior of households. An extension of the set of matching variables would reduce post-matching sample sizes too much. The more variables are considered for matching, the lower is the number of observations in the treatment and in the control group that have characteristics similar in *all* dimensions.

Table 3 about here

We employ a monotonic imbalance bounding class of matching methods called "Coarsened Exact Matching" (CEM), as recently suggested by Iacus *et al*. (2008). Matching is done without replacement. To assess the quality of the matching outcome, we compare, before and after matching, descriptive statistics of the matching variables in both the treatment and the control group. In addition, we provide two measures of imbalance suggested by Iacus *et al*. (2008). The first measure

[8] Ho *et al.* (2007) provide an extensive discussion of the issue of model dependence together with empirical examples.

$$(4) \qquad L_1(f,g) = \frac{1}{2} \cdot \sum_{l_1 \ldots l_k} \left| f_{l_1 \ldots l_k} - g_{l_1 \ldots l_k} \right|$$

gives the sum of absolute differences over all cells of a multivariate histogram. In Eq. (4), $f_{l_1 \ldots l_k}$ denote the relative frequencies of the categorical variables l_j for the treated households, and $g_{l_1 \ldots l_k}$ for the control households. These frequencies are obtained in three steps. First, the number of categories for each (continuous) variable is chosen. Then, the discretized variables are cross-tabulated separately for the treated and the control group. Finally, the *k*-dimensional relative frequency is computed. Perfect balance across all variables is achieved if $L_1(f,g) = 0$, whereas $L_1(f,g) = 1$ indicates perfect separation. Let the relative frequencies of the matched dataset be denoted by f^m and g^m; one hopes to find $\Delta L_1 = L_1(f,g) - L_1(f^m,g^m) > 0$, and the difference ΔL_1 can be interpreted as the increase in balance achieved as a result of matching.[9] The measure defined by (4) can also be quantified for each variable *j* separately, which we then denote by $L_1^{(j)}$, allows an assessment of the variable-specific imbalance.

Our second measure

$$(5) \qquad I_1^{(j)} = \bar{X}_{m_T,w}^{(j)} - \bar{X}_{m_C,w}^{(j)}, \qquad j=1,\ldots,k$$

is the difference in the means of variable j for the group of treated (m_T) and control units (m_C) matched, weighted by the matching weights assigned to each unit.[10]

4 Description of the database

4.1 Savings variables in the datasets

Our investigation is based on two data sources, the German Socio-Economic Panel (SOEP) and the SAVE Study. The SOEP is a longitudinal study, located at the DIW Berlin (German Institute for Economic Research). Starting in 1984, it surveys meanwhile more than 20,000 individuals in about 11,000 households every year.[11]

[9] See Blackwell *et al*. (2009, p. 6).
[10] We implemented the CEM in Stata using the command *cem*. For details, see Blackwell *et al*. (2009) and http://gking.harvard.edu/cem/.
[11] For details, see, e.g., Wagner et al. (2007) and the SOEP homepage at http://www.diw.de/en/soep.

The SOEP contains information on regular household saving and in some years it also reports whether a surveyed household member has a Riester contract or not. The exact wording of the survey question on saving reads as follows: "Do you usually have an amount of money left over at the end of the month that you can save for larger purchases, emergency expenses or to acquire wealth? If yes, how much?" (see SOEP online documentation: http://www.diw.de/english/questionnaires/33919.html). Hence, it is asked to state usual amounts intended for savings, including savings for old age but not to report accidental savings.

The survey question reported above has been used extensively in econometric analyses of household saving decisions in Germany, among others by Bauer and Sinning (2011), Fuchs-Schündeln (2008), Fuchs-Schündeln and Schündeln (2003), as well as Merkle and Zimmermann (1992). Nevertheless, it cannot be ruled out that savings may be under-reported in the SOEP data as Corneo *et al.* (2009) find that some respondents that claim to have a Riester contract declare zero savings. We therefore intend to provide substantial motivation for the usage of this savings variable. First, we run auxiliary regressions to find associations of our savings variable and well-established demographic and financial factors (the regression results are illustrated in the appendix). This plausibility check is in line with Alessie and Lusardi (1997) who also address the issue of measurement error in saving variables. Similar to them (and to the empirical literature on private savings), we find that the probability of declaring positive savings and the savings ratio increase with income and are higher for better educated households. The existence of children in the household has a negative association with savings. Furthermore, we are able to trace the non-linear effect of age on savings.

Second, we have a closer look on the Riester contract information contained in the dataset. Between the waves in which the SOEP asked whether the respondent has concluded a Riester contract, namely 2004, 2006, and 2007, we identify two groups of *switchers.* These are households with people who concluded a contract in between or with people who changed their response from *yes* to *no* in between. The latter group is most likely composed of people who have recalled an existing contract. This group is notably smaller than the group of those switching into a contract. Thus, we use the cross-section observations of 2004, 2006, and 2007 to regress the probability to save on the two switching indicators (whether the household switched into or out of a Riester contract). The results are displayed in Table A2 (appendix) and reveal two interesting aspects: In two of three year combinations, switching into a Riester contract is significantly associated with a higher probability to save. These associations are even robust against the inclusion of the set of control variables used in the proceeding

plausibility check. Also, we find in one year combination a negative and highly significant association between positive savings and switching out of a contract. Of course, these rudimentary correlation checks do not anticipate the results of the more elaborated empirical analyses in the following section. However, they give an important hint. If Riester savings were systematically neglected by the survey respondents, the probability to declare positive savings should be unaffected by the conclusion of a Riester contract.

Nevertheless, we take concerns about the quality of the saving variables in the SOEP seriously. Therefore, we enrich our analysis by using a second German panel database, the SAVE Study. Similarly to the SOEP, the SAVE data include a one-shot savings measure (overall amount saved in the previous year) and information on Riester contracts.[12] The questioning in SAVE has two advantages. First, the wording of the question is more explicit since it makes use of the term "saved" (instead of the more imprecise formulation in the SOEP questionnaire: "…amount of money left over […] that you can save …"). Second, the savings variable is asked after a longer list of questions on saving behavior in which different saving vehicles (e.g., life-insurance contracts) are explicitly named. We would therefore expect that the respondents are adequately prepared to give reliable estimates of their total savings.

To contrast the savings behavior of our data with those of the German population, we calculate mean savings ratios of the two datasets by year for all households in the datasets (using household weights to improve representativeness and excluding observations with missing information or unrealistically high saving rates of 100 percent and more).

Table 4 about here

As presented in the upper part of Table 4, the mean calculated saving ratios in both samples are very close. Starting at a level of 8.7 percent in 2000 (SOEP), we observe values between 6.6 and 7.9 percent between 2004 and 2007 (both datasets). Interestingly, mean savings ratios obtained from SAVE go a little below those obtained from the SOEP in all periods. Effectively, we cannot claim representativeness of these figures given missing information, censoring of the savings variables and a potential under-representativeness of income-rich households in the data. In fact, we intend to show that the two savings variables basically survey the same thing.

[12] On the SAVE Study see Börsch-Supan *et al.* (2008a). Essig (2005) discusses different savings measures and the reliability of the one-shot savings measure in the SAVE data. For further details, refer also to the SAVE questionnaires at the MEA homepage http://www.mea.uni-mannheim.de.

Our last step to give further insights in the savings variables we use is a comparison with the national accounts. Until here, we have focused on the saving ratio of each household so that the saving ratio of an income-poor household has been treated the same as the saving ratio of an income-rich household (if both households share the same household weight). Indeed, national accounts calculate saving ratios out of aggregated income and expenditure. Thus, the savings ratio of an income-rich household would receive a higher weight. Therefore, we calculate weighted incomes and savings amounts separately by year and dataset to receive aggregated saving ratios. These figures are displayed in the lower part of Table 4. We see that savings ratios increase in all years and reach a level of about nine percent in the SOEP and about eight percent in SAVE, respectively. Certainly, it is necessary to be aware that savings in national accounts and in our analysis differ in definition and methodology (as regards, e.g., household debt or asset decumulation). However, it is apparent that the savings variables that we take from the datasets are indeed applicable for our analysis.

Throughout the empirical analysis, we have to focus on the SOEP-based estimates because the number of households repeatedly participating in the SAVE Study is substantially smaller compared to the SOEP. In particular, sample sizes are not sufficient to ensure reliable matching results for the SAVE database. Another limitation of SAVE is that in the year 2000 – the year before the Riester reform – that database was still in its experimental stage. Hence, SAVE-based before-after reform comparisons cannot be taken seriously. Only an assessment of the impact of the so-called Riester steps on household savings is feasible, i.e., evaluations of the impact of the intertemporal rise in monetary incentives – higher allowances and tax deductions, but also higher required minimum savings efforts – on household savings. Taken these limitations together, we end up with regression results of two approaches (Approach 1 and Approach 5) in four year combinations (2005-2007, 2005-2008, 2006-2007, 2006-2008).

4.2 Saving behavior in treatment and control groups

Table 5 (SOEP) and Table 6 (SAVE) portrait the intertemporal variation of saving rates and also of the fraction of households with positive savings. All results are decomposed by treatment approach, post-reform period and dataset. Particularly, for the saving rate, five values are provided, each referring to a particular percentile (10th, 25th, 50th, 75th, 90th) of the ordered distribution of saving rates. All the estimates are derived by assigning the households to the treatment and control group in a particular post-reform period and then looking back at the pre-reform period.

Table 5 about here

Beginning with the Approaches 3-5 in the SOEP, the saving rates of treatment and control groups in the year 2000 are similarly distributed. For instance, 58 percent of the treatment subsample (Approach 3) in 2000 report positive savings with an unconditional mean saving ratio of 5.3 percent. The control group shows positive savings in 60 percent of the cases and has an unconditional mean saving ratio of 5.2 percent (both figures taken from the 2000-2004 comparison). Looking at the saving ratios of the different percentiles reveals further similarities.

Approach 1 and Approach 2 require a more detailed view. In both approaches, we observe similar conditional mean saving ratios and a comparable saving behavior at the lower and higher tails of the distribution (10th and 25th percentile as well as the 90th percentile in three of four year combinations). However, the fraction of saving households is notably larger in the treatment groups.

Table 6 about here

With SAVE, we can only compare the saving behavior of treated and control households in Approach 1 and Approach 5. In the first approach, the fractions of saving households and the mean saving ratios (conditional and unconditional) are close-by. In total, we observe similar distribution of saving ratios. On the contrary, saving ratios between households in treatment and control group differ more notably in the fifth approach. The fraction of saving households is remarkably low in the control group.

One may argue that differences in saving behavior in SOEP and SAVE indicate that at least one dataset does not get the level of savings exactly right. However, to infer the treatment effect it suffices that we estimate without systematic biases the differential changes in saving rates (see Attanasio and Brugiavini, 2003).

4.3 Time effects and group composition

To suitably identify treatment effects, we have to ensure that time effects are common across groups and that the composition of treatment and control groups is not affected by changes in covariates we cannot control for. As before, we start with discussing these aspects for the

more homogeneous subsamples in the Approaches 3-5. There is no reason to argue that macro effects between the observation points could have affected treatment and control groups in a different manner. Since we use age, employment status, household type, income, and debt repayments as control variables, we state that the most influential observable factors are adequately accounted for. In addition, we believe that unobservable factors (such as saving preferences) are similarly distributed between the treatment and control samples in the third approach, and can be taken as time-invariant for the Approaches 4 and 5.

This issue is more complex in the first two approaches. By definition, employment states are crucial for the assignment of the subgroups in these two approaches. However, we capture changes in the economic environment by controlling for household income and debt repayments. Moreover, the Rürup pension scheme with a possibly stronger impact on self-employed (which we mostly find in the control groups) was only introduced in 2005 and started slowly.[13] Finally, unobserved factors with a potential influence on savings are accounted for by taking first-differences. In Table A3 (SOEP) and A4 (SAVE) in the appendix, we display the compositions of treatment and control groups for all approaches, all year-combinations, and all relevant covariates in detail.

5 Results

5.1 Panel regressions: random-effects and first-differences

For each of our five approaches and intertemporal comparisons, three model specifications are estimated. The model specifications differ by the set of control variables. For each intertemporal comparison, the first specification contains the estimates pertaining to a regression specification without any further control variable. In the second specification, basic socio-demographic household characteristics are included as controls. The third specification uses the full set of control variables which comprises household types, age and employment type of the head of household, income, and loan repayments. Depending on the different approaches, *treatment* can be linked to a certain employment type (for instance, bluecollar workers are eligible so that we would not find a household in which the household head's employment status is bluecollar worker in the control group of the first approach), so that the basic set of controls (second specification) includes all covariates that are not used to distinguish between treatment and control groups.

[13] With about 153,200 contracts in 2005 (German Bundestag, 2008).

Table 7 about here

The *treatment* coefficients are of special interest for our purposes. Since the random-effects regressions build on a tobit model, the regression coefficients cannot be interpreted as marginal effects. Therefore, we calculate marginal effects and concentrate on the marginal effect of the treatment dummy on the unconditional expected value of the savings ratio (displayed in Table 7).

The results are not unambiguous. While we find insignificant treatment effects in all specifications of Approach 3 and in most specifications of Approach 4 and Approach 5, savings appear to respond to *treatment* in some specifications of the first two approaches (mostly in 2000-2005 comparisons). Among the control variables, household composition, unemployment, income, and loan repayments have a robust effect on saving rates (results not reported).

However, we have already discussed the limitations of the different approaches in the third section. Even if we argue that households do not self-select into or out of the treatment groups, we have to point out that unobserved characteristics of the control groups (e.g., being self-employed) in the first and second approach are possibly correlated with saving preferences. Similarly, *treatment* households in the fourth and fifth approach (those with concluded contracts) are likely to differ in their savings behavior from households who refused to conclude such a contract. Therefore, we believe that the third approach is indeed the most reliable one and that positive treatment effects in the remaining approaches are possibly influenced by self-selection and endogeneity bias.

In the first-differences regressions, the *treatment* coefficients can directly be interpreted as the marginal effect of the treatment status on the saving ratio. The results displayed in Table 8 reveal a clear picture. Treatment is insignificant in all specifications. We interpret these findings as strong evidence against the effectiveness to mobilize private savings in the treatment groups under consideration. Moreover, since the first-differences results are not driven by (time-invariant) unobserved heterogeneity, we prefer them to the (mixed) random-effects results discussed in the previous subsection.

The influence of the control variables is as expected. As before, household composition, income, unemployment, and debt repayments affect saving decisions (results not reported).

Table 8 about here

5.2 Results from matching

To allow an assessment of the degree of imbalance in the original unmatched data and the data after matching, for each of our five approaches, Table 9 reports summary statistics allowing, for each matching variable, an assessment of the degree of imbalance before and after matching.

Table 9 about here

By way of an example, consider Approach 1 when the inter-temporal comparison refers to periods 2000 and 2005. For each of the matching variables, the columns entitled $L_1^{(j)}$ and $I_1^{(j)}$ give the estimates of the variable-specific imbalance measures after and before matching. In the adjacent columns, imbalances between the treated and controls, the minimum, the three quantile means, and the maximum are reported. Thus, the value 0.080 appearing in column "$L_1^{(j)}$, after" for the variable *age* indicates a moderate imbalance between the treat and control units matched, which is substantially lower than the estimate 0.311 for the non-matched observations that appears in column "$L_1^{(j)}$, *before*". Also the second measure, $I_1^{(j)}$, points to a substantial decrease of imbalance for the variable *age* – as shown by the value –0.248 for the matched units as compared to -3.138 for the units before matching. Next to the variable-specific imbalance measures we report the change in the global imbalance measure, ΔL_1, which indicates that the matching algorithm was effective in increasing the balance over all the matching variables. It is transparent that the matching procedure has been effective in reducing the global imbalance across all variables as well as variable specific imbalances. This applies to all approaches.

We are now in a position to inspect whether our previous conclusions on the effectiveness of the Riester scheme hold for the matched units. The results are summarized in the column *Average treatment effect* in Table 10. The treatment effect is insignificant in nine out of twelve cases. Only in three cases the treatment effect is significant and carries the correct sign (Approach 1 and Approach 2 only). On the contrary, treatment cannot explain private savings in all year-combinations of the Approach 3-5. Hence, also for the units

matched we cannot identify a stimulating effect of the Riester scheme on the propensity to save in most of the cases.

A limitation of the matching approach is that it leads to small sample sizes. Small samples raise the question of whether the conclusions drawn from the matching approach are representative for the underlying overall population. This explains why it was useful to combine the matching approach with a panel regression analysis, so as to assess the robustness of the empirical results.

Table 10 about here

5.3 Results from SAVE

As mentioned above, the SOEP saving variable is possibly an imperfect measure of a household's savings. Therefore, we have conducted a regression analysis using a second dataset, the SAVE Study. While the SAVE Study was explicitly designed to investigate saving behavior, it only allows for an analysis of the effectiveness of the so-called Riester steps, i.e. the increase of the subsidy rate after 2005.

The results from random-effects panel regressions based on the SAVE data are exhibited in Table 11. Those results are in line with those obtained applying the same methodology to the SOEP data (Table 4). As shown in Table 11, we find insignificant treatment effects in all specifications of Approach 1 and Approach 5.

Table 11 about here

Table 11 displays results from first-differences which are in line with the random-effects result discussed before. In both approaches and all specifications, *treatment* does not show a significant impact on savings. Our previous conclusion from the SOEP sample that Riester subsidies have no detectable mobilization effects on private savings can therefore be enriched with the SAVE results. Mostly insignificant treatment effects that we find by comparing SOEP savings before and after the introduction of the reform come along with insignificant treatment effects that we find by comparing savings in SAVE along the progression of the reform.

6 Concluding remarks

The Riester scheme is the central pillar of governmental promotion of private retirement saving in Germany. In this paper, we conduct a comprehensive treatment analysis of the Riester scheme so as to assess its effectiveness in raising private household savings. The introduction of the Riester allowances and tax deductions can be interpreted as a natural experiment and we investigate how the savings of treated household have evolved as compared to the savings of control households. In order to check the robustness of our results, we employ panel regressions and matching methods to reduce problems of unobserved heterogeneity and sample selection bias. Several model specifications as well as time periods are examined and two datasets are used, the German SOEP and the SAVE Study.

We provide a detailed discussion of strengths and weaknesses of the different treatment approaches we apply and we critically examine the reliability of the savings variables contained in the two datasets we use.

Despite the variety of estimation methods and datasets, the obtained results are fairly stable. The effect of the Riester scheme on mobilizing private savings is mostly insignificant. This holds for the first-differences regressions and matching results of all treatment approaches, year combinations, and model specifications as well as for most random-effects results. In some random-effects specifications, we find indeed significantly positive treatment effects which we attribute to self-selection and endogeneity bias. Our results are similar for evaluations of the reform itself (comparing savings before and after the introduction of the reform with SOEP data) and of the progression of the reform steps (comparing savings at different points in time during the reform with SAVE data).

Apparently, many private households that would have saved also in the absence of the Riester scheme simply allocated some of their savings to Riester contracts. In this way, those households can improve their future living standards without the pain of reducing current consumption. The likely counterpart of those windfall gains is an increase in public debt, which calls for larger primary surpluses in the future. This suggests that a major effect of the Riester scheme is to substitute future increases in social security contributions with future tax increases.

The ineffectiveness of saving incentives may be more pronounced in Germany than in other countries. German households traditionally display a relatively high saving rate. Furthermore, all compulsorily insured persons regularly receive notification about the likely amount of pension benefit that they are going to receive as a retiree. Alternative long-term financial instruments, e.g., life insurance, are common and well known by the population. In

such a situation, the rationale for subsidizing certified retirement plans is rather weak. Our empirical results corroborate the view that there may be better uses of taxpayer money for old-age provision than the Riester scheme.

References

Alessie, R. and Lusardi, A., 1997. Saving and income smoothing: Evidence from panel data. *European Economic Review* 41(7), 1251-1279.

Antolín, P., de Serres, A., and de la Maisonneuve, C., 2004. Long-Term Budgetary Implications of Tax-Favoured Retirement and Saving Plans. OECD Economic Studies #39.

Attanasio, O. P., and Brugiavini, A., 2003. Social Security and Households' Saving. *Quarterly Journal of Economics* 118(3), 1075-1119.

Bauer, T. K. and Sinning, M., 2011. The Savings Behavior of Temporary and Permanent Migrants in Germany. *Journal of Population Economics* 24(2), 421-449.

Benjamin, D. J., 2003. Does 401(k) Eligibility Increase Saving? Evidence from Propensity Score Subclassification. *Journal of Public Economics* 87(5-6), 1259-1290.

Blackwell, M., Iacus, S., King, G., and Porro, G., 2009. cem: Coarsened Exact Matching in Stata, downloaded from http://gking.harvard.edu/files/cem-stata.pdf.

Börsch-Supan, A., Coppola, M., Essig, L., Eymann, A., and Schunk, D., 2008a. The German SAVE Study - Design and Results. MEA Studies No. 6, Mannheim.

Börsch-Supan, A., Reil-Held, A., and Schunk, D., 2008b. Saving Incentives, Old-age Provision and Displacement Effects: Evidence from the Recent German Pension Reform. *Journal of Pension Economics and Finance* 7, 295-319.

Bräuninger, D., 2005. Spürbare Rentenlücken trotz Reformen. Deutsche Bank Research, Aktuelle Themen, Demografie Spezial # 312.

Chernozhukov, V., and Hansen, C., 2004. The Effects of 401(k) Participation on the Wealth Distribution: An Instrumental Quantile Regression Analysis. *Review of Economics and Statistics* 86(3), 735-751.

Corneo, G., Keese, M., and Schröder, C., 2009. The Riester Scheme and Private Savings: An Empirical Analysis Based on the German SOEP, *Journal of Applied Social Science Studies (Schmollers Jahrbuch)* 129 (2), 321-332.

Duflo, E., Gale, W., Liebman, J., Orszag, P., and Saez, E., 2007. Savings Incentives for Low- and Moderate-Income Families in the United States: Why is the Saver's Credit Not More Effective? *Journal of the European Economic Association* 5(2-3), 647-661.

Engen, E. M., Gale, W. G., and Scholz, J. K., 1994. Do Saving Incentives Work? Brookings Papers on Economic Activity 25(1994-1), 85-180.

Essig, L., 2004. Measures for Savings and Saving Rates in the German SAVE Data Set. MEA Discussion Paper #086-05.

Federal Ministry of Labour and Social Affairs (ed.), 2006. Zusätzliche Altersvorsorge. Bonn: BMAS, Referat Information, Publikation, Redaktion.

Federal Statistical Office (ed.), 2007. Verbraucherpreisindizes für Deutschland. Lange Reihen - Revisionsbericht, Fachserie 17, Reihe 7. Wiesbaden.

Federal Statistical Office (ed.), 2009. Staatliche Förderung der Riester-Rente 2004. Wiesbaden.

Fuchs-Schündeln, N., 2008. The Response of Household Saving to the Large Shock of German Reunification. *American Economic Review* 98(5), 1798-1828.

Fuchs-Schündeln, N., and Schündeln, M., 2003. The Savings Behavior of East and West Germans -Theoretical Predictions and Empirical Evidence. *Journal of Applied Social Science Studies (Schmollers Jahrbuch)* 123(1), 209-219.

German Bundestag (ed.), 2008. Antwort der Bundesregierung auf die Kleine Anfrage der Abgeordneten [...] - Verbraucherschutz bei der geförderten Altersvorsorge. BT-Drucksache 16/11194.

Ho, D.E., Imai, K., King, G., and Stuart, E.A., 2007. Matching as Nonparametric Preprocessing for Reducing Model Dependence in Parametric Causal Inference, *Political Analysis* 15, 199-236.

Iacus, S.M., King, G., and Porro, G., 2008. Matching for Causal Inference without Balance Checking. Harvard University Working Paper, downloaded from: http://gking.harvard.edu/files/cem.pdf.

Kemmerling, A., and M. Neugart, 2009. Financial Market Lobbies and Pension Reform. *European Journal of Political Economy* 25(2), 163-173.

Merkle, L., and Zimmermann, K.F., 1992. Savings, Remittances, and Return Migration. *Economics Letters* 38(1), 77-81.

Pfarr, C., and Schneider, U., 2009. Demand Inducement, Crowding In and the German Riester Pension Scheme. MPRA Paper #17759.

Poterba, J. M., Venti, S. F., and Wise, D. A., 1995. Do 401(k) Contributions Crowd Out other Personal Saving? *Journal of Public Economics* 58(1), 1-32.

Rubin, D.B., 1987. Multiple Imputation for Nonresponse in Surveys. New York: Wiley.

Schulze, I. and Jochem, S., 2007. Germany: Beyond Policy Gridlock, in: Immergut, E., Anderson, K., and Schulze, I. (eds.), 2007. The Handbook of West European Pension Politics. Oxford: Oxford University Press, 660-710.

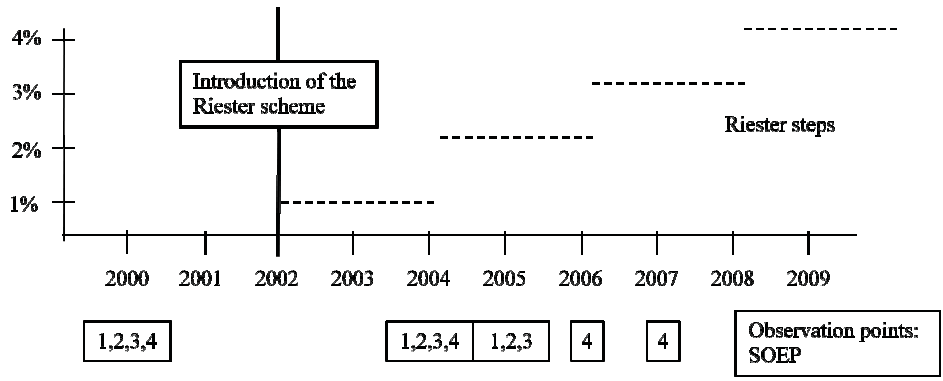
Stolz, U. and Rieckhoff, C., 2008. Förderung der zusätzlichen Altersvorsorge für das Beitragsjahr 2005 durch die ZfA. *RVaktuell* 9, 267-273.

Wagner, G. G., Frick, J. R. and Schupp, J., 2007. The German Socio-Economic Panel Study (SOEP): Scope, Evolution and Enhancements. *Journal of Applied Social Science Studies (Schmollers Jahrbuch)* 127 (1), 139-170.

Yoo, K.-Y. and de Serres, A., 2004. Tax Treatment of Private Pension Savings in OECD Countries and the Net Tax Cost Per Unit of Contribution to Tax-Favoured Schemes, OECD Economics Department Working Papers #406.

Figures

Figure 1. The Riester scheme and observation points (savings) in the datasets



Note. Own illustration. Savings in SAVE refer to the previous year. Example: 2007 savings are taken from 2008 SAVE data.

Tables

Table 1. Fiscal costs of the Riester scheme

	2002	2003	2004	2005	2006	2007	2008
# Riester contracts (in mio.)	3.37	3.92	4.19	5.63	8.05	10.76	12.15
Allowances (in mio. Euro)	146.8	173.9	384.9	521.9	*1,114.3*	*1,488.9*	*2,241.8*
Tax deductions (in mio. Euro)	38.5	53.5	107.8	*147.2*	*314.3*	*420.1*	*632.4*
Total subsidies (in mio. Euro)	185.3	227.4	492.7	*669.1*	*1,428.6*	*1,909.0*	*2,874.2*

Note. Italic figures are extrapolations based on the figures of the previous years. Source: Number of Riester contracts: Federal Ministry of Labour and Social Affairs (http://www.bmas.bund.de); allowances/tax deductions: Federal Statistical Office (2009), Stolz and Rieckhoff (2008).

Table 2. Definitions of treatment and control group

Approach	Treatment group	Control group
1	Households eligible for Riester	Households not eligible for Riester
2	Households eligible for Riester Equivalent income below mean	Households not eligible for Riester Equivalent income below mean
3	Two married adults, two children Equivalent income below mean	Two married adults, one child Equivalent income below mean
4	Household head with a Riester contract Equivalent income below mean	Household head without a Riester contract but eligible Equivalent income below mean
5	Household with a Riester contract Equivalent income below mean	Household without a Riester contract but eligible Equivalent income below mean

Note. Own illustration.

Table 3. Matching variables and their coarsened categories

Matching variable	Approach 1	2	3	4 and 5
Household type	Dummies for HH-type 1-6	Dummies for HH-type 1-6	Dummies for HH-type 4-6	Dummies for HH-type 1-6
Income in 1,000	0-1; 1-2; 2-3; 3-4; 4-5; 5-6; 6-8; 8-10; 10-12; 12-14; 14-16	0-1; 1-1.5, 1.5-2; 2-2.5; 2,5-3; 3-4; 4-5; 5-6; 6-8	0-1; 1-1.5; 1-5-2; 2-2.5; 2.5-3; 3-4	0-1; 1-1.5, 1.5-2; 2-2.5; 2,5-3; 3-4; 4-5; 5-6; 6-8
Age (household head)	0-20; 20-30; 30-35; 35-40; 40-45; 45-50; 50-55; 55-60	0-20; 20-30; 30-35; 35-40; 40-45; 45-50; 50-55; 55-60	0-20; 20-30; 30-35; 35-40; 40-45; 45-50; 50-55; 55-60	0-20; 20-30; 30-35; 35-40; 40-45; 45-50; 50-55; 55-60

Note. Own illustration. HH-type 1: singles; HH-type 2: couples without children; HH-type 3: single parents; HH-type 4: couples with children aged 16 and younger; HH-type 5: couples with cohabiting children aged older than 16; HH-type 6: couples with younger and older children.

Table 4. Saving ratios in the two datasets and in the national accounts

	2000	2004	2005	2006	2007
Not aggregated					
SOEP	8.68	7.43	7.89	7.58	7.40
SAVE	---	7.01	6.89	6.76	6.98
Aggregated					
SOEP	9.68	8.90	9.26	9.21	9.34
SAVE	---	7.04	7.96	7.76	8.10
National accounts	9.2	10.4	10.5	10.6	10.8

Note. Savings taken from SAVE data refer to the previous year. Source: SOEP, SAVE, Federal Statistical Office at http://www.destatis.de.

Table 5. Savings variables in treatment and control groups: SOEP data

Approach	Year combi-nation	Group	Fraction: positive savings	Mean saving ratio		Saving ratio by percentile of ordered saving rates		
				uncond.	cond.	50th	75th	90th
1	2000	Treat	0.641	7.687	11.985	4.998	11.255	20.011
		Control	0.441	6.488	14.718	0.000	9.661	20.004
	2004	Treat	0.606	7.053	11.637	4.344	10.493	18.657
		Control	0.392	5.381	13.710	0.000	7.926	16.667
	2000	Treat	0.644	7.763	12.048	5.000	11.362	20.043
		Control	0.423	5.762	13.612	0.000	8.334	18.182
	2005	Treat	0.626	7.393	11.816	4.706	10.870	19.201
		Control	0.368	4.522	12.284	0.000	5.470	14.286
2	2000	Treat	0.497	4.744	9.553	0.000	7.419	13.043
		Control	0.302	3.149	10.432	0.000	3.392	9.871
	2004	Treat	0.451	4.161	9.230	0.000	6.766	12.327
		Control	0.292	2.667	9.120	0.000	3.261	11.249
	2000	Treat	0.511	5.102	9.988	1.757	7.998	13.512
		Control	0.300	3.154	10.513	0.000	2.942	12.171
	2005	Treat	0.466	4.634	9.943	0.000	7.003	13.636
		Control	0.260	2.099	8.074	0.000	2.069	7.913
3	2000	Treat	0.577	5.267	9.127	3.657	8.001	13.371
		Control	0.598	5.218	8.730	3.570	7.697	12.728
	2004	Treat	0.557	4.671	8.385	2.759	7.862	12.931
		Control	0.512	4.391	8.581	1.802	7.029	11.433
	2000	Treat	0.596	5.606	9.404	4.052	8.977	13.406
		Control	0.557	5.146	9.231	2.967	7.894	12.821
	2005	Treat	0.551	5.014	9.096	2.746	7.971	13.706
		Control	0.511	4.724	9.252	1.920	7.143	13.310
4	2000	Treat	0.539	4.828	8.956	2.086	8.022	12.501
		Control	0.497	4.797	9.647	0.000	7.406	13.260
	2004	Treat	0.477	3.945	8.264	0.000	6.250	10.862
		Control	0.452	4.235	9.379	0.000	6.897	12.763
	2000	Treat	0.552	5.394	9.775	3.092	8.800	13.157
		Control	0.511	5.081	9.951	1.724	7.977	13.888
	2006	Treat	0.488	4.785	9.809	0.000	7.237	13.976
		Control	0.437	4.101	9.395	0.000	6.561	12.590
	2000	Treat	0.583	5.804	9.962	3.572	8.751	14.585
		Control	0.499	4.847	9.721	0.000	7.738	13.406
	2007	Treat	0.530	4.798	9.045	2.166	8.084	13.401
		Control	0.431	3.685	8.551	0.000	5.999	11.737
5	2000	Treat	0.566	4.948	8.736	2.407	7.793	12.870
		Control	0.492	4.780	9.722	0.000	7.406	13.157
	2004	Treat	0.487	4.389	9.006	0.000	6.701	12.387
		Control	0.447	4.149	9.277	0.000	6.818	12.232
	2000	Treat	0.553	5.220	9.442	2.943	8.500	13.441
		Control	0.506	5.105	10.081	1.540	8.001	13.634
	2006	Treat	0.507	4.663	9.193	1.656	7.209	13.514
		Control	0.426	4.143	9.728	0.000	6.452	12.821
	2000	Treat	0.568	5.573	9.809	3.338	8.695	13.760
		Control	0.485	4.648	9.585	0.000	7.406	13.329
	2007	Treat	0.523	4.567	8.726	2.039	7.769	13.159
		Control	0.417	3.643	8.738	0.000	5.882	11.765

Note. Own calculations. Saving ratios for 10th and 25th percentile not reported because they are zero in all rows. Data source: SOEP 2000 and 2004-2007.

Table 6. Savings variables in treatment and control groups: SAVE data

Treat-ment approach	Year combi-nation	Group	Fraction: positive savings	Mean saving ratio		Saving ratio by percentile of ordered saving rates		
				uncond.	cond.	50th	75th	90th
1	2005	Treat	0.460	5.252	11.426	0.000	7.160	15.486
		Control	0.500	5.126	10.250	1.563	7.632	13.976
	2007	Treat	0.449	5.137	11.434	0.000	6.966	16.381
		Control	0.323	3.772	11.656	0.000	4.147	13.754
	2005	Treat	0.488	5.518	11.298	0.000	8.098	15.654
		Control	0.450	6.230	13.851	0.000	8.374	17.734
	2008	Treat	0.488	4.890	10.011	0.000	7.927	14.589
		Control	0.367	5.736	15.653	0.000	8.333	17.647
	2006	Treat	0.499	6.076	12.173	0.121	8.909	17.460
		Control	0.437	5.766	13.195	0.000	8.374	21.244
	2007	Treat	0.482	5.352	11.100	0.000	8.294	16.667
		Control	0.392	4.652	11.854	0.000	7.399	14.621
	2006	Treat	0.489	5.935	12.129	0.000	8.632	17.888
		Control	0.429	5.935	13.828	0.000	8.379	21.714
	2008	Treat	0.489	5.052	10.339	0.000	7.884	15.096
		Control	0.359	5.252	14.618	0.000	5.480	17.660
5	2005	Treat	0.476	4.589	11.426	0.000	9.100	16.167
		Control	0.256	1.559	10.250	0.000	0.833	5.651
	2007	Treat	0.450	4.855	11.434	0.000	6.944	15.238
		Control	0.246	2.274	11.656	0.000	0.208	7.292
	2005	Treat	0.484	4.368	11.298	0.093	7.866	13.287
		Control	0.279	2.911	13.851	0.000	2.741	10.163
	2008	Treat	0.564	4.193	10.011	1.249	5.409	13.519
		Control	0.213	2.111	15.653	0.000	0.000	8.545
	2006	Treat	0.464	4.875	12.173	0.000	5.652	10.625
		Control	0.301	2.978	13.195	0.000	2.755	9.450
	2007	Treat	0.425	3.924	11.100	0.000	6.213	11.925
		Control	0.313	2.837	11.854	0.000	2.317	8.721
	2006	Treat	0.494	5.107	12.129	0.217	6.528	10.482
		Control	0.287	2.891	13.828	0.000	2.217	8.572
	2008	Treat	0.487	3.961	10.339	0.128	5.792	13.526
		Control	0.247	2.674	14.618	0.000	0.556	9.187

Note. Own calculations. Calculations based on five multiply imputed datasets. Saving ratios for 10^{th} and 25^{th} percentile not reported because they are zero in all rows. Data source: SAVE 2005-2008.

Table 7. Random-effects tobit regressions (marginal effects)

Approach	Periods		Specification (1)	(2)	(3)
1	00-04	Treatment	1.771***	1.259**	0.601
			(0.613)	(0.604)	(0.626)
		Households		4,291 [95.67]	
	00-05	Treatment	2.772***	2.647***	2.007***
			(0.685)	(0.680)	(0.702)
		Households		4,021 [95.95]	
2	00-04	Treatment	1.112*	0.457	0.463
			(0.621)	(0.614)	(0.634)
		Households		2,027 [94.77]	
	00-05	Treatment	2.164***	1.553**	1.725**
			(0.733)	(0.723)	(0.744)
		Households		1,958 [94.89]	
3	00-04	Treatment	0.399	-0.485	-0.591
			(0.428)	(0.428)	(0.437)
		Households		755 [66.09]	
	00-05	Treatment	0.176	-0.568	-0.610
			(0.460)	(0.469)	(0.483)
		Households		703 [66.57]	
4	00-04	Treatment	-0.161		-0.041
			(0.373)		(0.368)
		Households		1,862 [13.05]	
	00-06	Treatment	0.608		0.251
			(0.387)		(0.360)
		Households		1,652 [19.85]	
	00-07	Treatment	0.892**		0.641*
			(0.383)		(0.363)
		Households		1,424 [24.23]	
5	00-04	Treatment	0.143		0.217
			(0.345)		(0.339)
		Households		1,868 [16.92]	
	00-06	Treatment	0.735**		0.263
			(0.362)		(0.335)
		Households		1,658 [25.09]	
	00-07	Treatment	0.791**		0.425
			(0.339)		(0.320)
		Households		1,499 [31.35]	

Note. (1): basic specification; (2): reduced set of controls; (3): full set of controls.*** p<0.01, ** p<0.05, * p<0.1. Standard errors in parentheses. Fraction of treated households in brackets. Marginal effects for the unconditional expected value of savings ratio. Data source: SOEP.

Table 8. First-differences regression results (marginal effects)

Approach	Periods		Specification (1)	(2)	(3)
1	00-04	Treatment	0.473	0.219	-0.097
			(0.691)	(0.677)	(0.698)
	00-05	Treatment	0.870	0.839	0.583
			(0.759)	(0.744)	(0.769)
2	00-04	Treatment	-0.101	-0.154	0.034
			(0.684)	(0.681)	(0.710)
	00-05	Treatment	0.587	0.667	1.060
			(0.798)	(0.795)	(0.835)
3	00-04	Treatment	0.231	0.071	-0.033
			(0.513)	(0.529)	(0.537)
	00-05	Treatment	-0.170	-0.096	-0.114
			(0.550)	(0.576)	(0.594)
4	00-04	Treatment	-0.321		-0.320
			(0.469)		(0.466)
	00-06	Treatment	0.370		0.240
			(0.455)		(0.452)
	00-07	Treatment	0.156		0.108
			(0.447)		(0.447)
5	00-04	Treatment	0.072		0.073
			(0.421)		(0.419)
	00-06	Treatment	0.405		0.326
			(0.426)		(0.425)
	00-07	Treatment	-0.000		-0.021
			(0.398)		(0.401)

Note. Linear model. (1): basic specification; (2): reduced set of controls; (3): full set of controls.*** p<0.01, ** p<0.05, * p<0.1. Standard errors in parentheses. Number of observations: see Table 5. Data source: SOEP.

Table 9. Measures of imbalance in the distributions of treated and control units of matching

	Variables	Year	$L_1^{(j)}$		$I_1^{(j)}$		Between-group differences by quantiles				
			after	before	after	before	0	25	50	75	100
Approach 1 2000 vs 2004 $\Delta L_1 = 0.088$	HH-type 1	2000	0.000	0.290	0.000	-0.290	0.000	0.000	0.000	0.000	0.000
		2004	0.000	0.310	0.000	-0.310	0.000	0.000	0.000	0.000	0.000
	HH-type 2	2000	0.000	0.108	0.000	0.108	0.000	0.000	0.000	0.000	0.000
		2004	0.000	0.095	0.000	0.095	0.000	0.000	0.000	0.000	0.000
	HH-type 3	2000	0.000	0.093	0.000	-0.093	0.000	0.000	0.000	0.000	0.000
		2004	0.000	0.118	0.000	-0.118	0.000	0.000	0.000	0.000	0.000
	HH-type 4	2000	0.000	0.173	0.000	0.173	0.000	0.000	0.000	0.000	0.000
		2004	0.000	0.203	0.000	0.203	0.000	0.000	0.000	0.000	0.000
	HH-type 5	2000	0.000	0.058	0.000	0.058	0.000	0.000	0.000	0.000	0.000
		2004	0.000	0.085	0.000	0.085	0.000	0.000	0.000	0.000	0.000
	HH-type 6	2000	0.000	0.043	0.000	0.043	0.000	0.000	0.000	0.000	0.000
		2004	0.000	0.045	0.000	0.045	0.000	0.000	0.000	0.000	0.000
	Income	2000	0.169	0.331	0.033	0.511	0.000	-0.014	0.044	0.000	-1.102
		2004	0.211	0.389	0.051	0.771	-0.010	0.093	-0.069	0.325	0.605
	Age	2000	0.206	0.197	-0.244	-1.710	-3.000	-1.000	2.000	0.000	0.000
Approach 1 2000 vs 2005 $\Delta L_1 = 0.102$	HH-type 1	2000	0.000	0.261	0.000	-0.261	0.000	0.000	0.000	0.000	0.000
		2005	0.000	0.332	0.000	-0.332	0.000	0.000	0.000	0.000	0.000
	HH-type 2	2000	0.000	0.088	0.000	0.088	0.000	0.000	0.000	0.000	0.000
		2005	0.000	0.092	0.000	0.092	0.000	0.000	0.000	0.000	0.000
	HH-type 3	2000	0.000	0.117	0.000	-0.117	0.000	0.000	0.000	0.000	0.000
		2005	0.000	0.135	0.000	-0.135	0.000	0.000	0.000	0.000	0.000
	HH-type 4	2000	0.000	0.194	0.000	0.194	0.000	0.000	0.000	0.000	0.000
		2005	0.000	0.227	0.000	0.227	0.000	0.000	0.000	0.000	0.000
	HH-type 5	2000	0.000	0.049	0.000	0.049	0.000	0.000	0.000	0.000	0.000
		2005	0.000	0.077	0.000	0.077	0.000	0.000	0.000	0.000	0.000
	HH-type 6	2000	0.000	0.047	0.000	0.047	0.000	0.000	0.000	0.000	0.000
		2005	0.000	0.072	0.000	0.072	0.000	0.000	0.000	0.000	0.000
	Income	2000	0.180	0.344	0.028	0.545	-0.053	0.110	0.088	-0.033	0.044
		2005	0.244	0.433	0.070	0.809	0.000	0.149	-0.117	0.190	-0.778
	Age	2000	0.080	0.239	-0.248	-3.138	-2.000	0.000	1.000	-1.000	0.000
Approach 2 2000 vs 2004 $\Delta L_1 = 0.350$	HH-type 1	2000	0.000	0.261	0.000	-0.261	0.000	0.000	0.000	0.000	0.000
		2004	0.000	0.287	0.000	-0.287	0.000	0.000	0.000	0.000	0.000
	HH-type 2	2000	0.000	0.027	0.000	0.027	0.000	0.000	0.000	0.000	0.000
		2004	0.000	0.016	0.000	0.016	0.000	0.000	0.000	0.000	0.000
	HH-type 3	2000	0.000	0.102	0.000	-0.102	0.000	0.000	0.000	0.000	0.000
		2004	0.000	0.152	0.000	-0.152	0.000	0.000	0.000	0.000	0.000
	HH-type 4	2000	0.000	0.271	0.000	0.271	0.000	0.000	0.000	0.000	0.000
		2004	0.000	0.278	0.000	0.278	0.000	0.000	0.000	0.000	0.000
	HH-type 5	2000	0.000	0.018	0.000	0.018	0.000	0.000	0.000	0.000	0.000
		2004	0.000	0.082	0.000	0.082	0.000	0.000	0.000	0.000	0.000
	HH-type 6	2000	0.000	0.047	0.000	0.047	0.000	0.000	0.000	0.000	0.000
		2004	0.000	0.063	0.000	0.063	0.000	0.000	0.000	0.000	0.000
	Income	2000	0.109	0.374	0.031	0.555	0.000	0.104	0.028	0.009	0.804
		2004	0.170	0.452	0.027	0.793	-0.002	-0.010	0.174	-0.023	0.223
	Age	2000	0.093	0.249	-0.141	-2.571	-3.000	0.000	0.000	-1.000	0.000
Approach 2 2000 vs 2005 $\Delta L_1 = 0.270$	HH-type 1	2000	0.000	0.253	0.000	-0.253	0.000	0.000	0.000	0.000	0.000
		2005	0.000	0.314	0.000	-0.314	0.000	0.000	0.000	0.000	0.000
	HH-type 2	2000	0.000	0.001	0.000	0.001	0.000	0.000	0.000	0.000	0.000
		2005	0.000	0.010	0.000	-0.010	0.000	0.000	0.000	0.000	0.000
	HH-type 3	2000	0.000	0.148	0.000	-0.148	0.000	0.000	0.000	0.000	0.000
		2005	0.000	0.151	0.000	-0.151	0.000	0.000	0.000	0.000	0.000
	HH-type 4	2000	0.000	0.313	0.000	0.313	0.000	0.000	0.000	0.000	0.000
		2005	0.000	0.283	0.000	0.283	0.000	0.000	0.000	0.000	0.000
	HH-type 5	2000	0.000	0.037	0.000	0.037	0.000	0.000	0.000	0.000	0.000
		2005	0.000	0.102	0.000	0.102	0.000	0.000	0.000	0.000	0.000
	HH-type 6	2000	0.000	0.049	0.000	0.049	0.000	0.000	0.000	0.000	0.000
		2005	0.000	0.091	0.000	0.091	0.000	0.000	0.000	0.000	0.000
	Income	2000	0.199	0.355	0.015	0.591	-0.055	0.002	0.138	-0.166	0.220
		2005	0.130	0.461	0.018	0.808	0.000	0.004	0.100	-0.029	-0.170
	Age	2000	0.109	0.291	-0.352	-4.490	-2.000	0.000	1.000	-1.000	0.000

Table continues

	Variables	Year	$L_1^{(j)}$		$I_1^{(j)}$		Between-group differences by quantiles				
			after	before	after	before	0	25	50	75	100
Approach 3	HH-type 4	2000	0.000	0.193	0.000	0.193	0.000	0.000	0.000	0.000	0.000
2000 vs 2004		2004	0.000	0.125	0.000	0.125	0.000	0.000	0.000	0.000	0.000
$\Delta L_1 = 0.129$	HH-type 5	2000	0.000	0.220	0.000	-0.220	0.000	0.000	0.000	0.000	0.000
		2004	0.000	0.334	0.000	-0.334	0.000	0.000	0.000	0.000	0.000
	HH-type 6	2000	0.000	0.026	0.000	0.026	0.000	0.000	0.000	0.000	0.000
		2004	0.000	0.208	0.000	0.208	0.000	0.000	0.000	0.000	0.000
	Income	2000	0.205	0.225	0.026	0.159	0.099	0.017	-0.094	0.016	-0.358
		2004	0.211	0.305	0.051	0.394	-0.406	-0.004	0.002	0.096	-0.004
	Age	2000	0.141	0.292	-0.334	-3.723	-2.000	-1.000	-1.000	0.000	0.000
Approach 3	HH-type 4	2000	0.000	0.259	0.000	0.259	0.000	0.000	0.000	0.000	0.000
2000 vs 2005		2005	0.000	0.181	0.000	0.181	0.000	0.000	0.000	0.000	0.000
$\Delta L_1 = 0.144$	HH-type 5	2000	0.000	0.202	0.000	-0.202	0.000	0.000	0.000	0.000	0.000
		2005	0.000	0.408	0.000	-0.408	0.000	0.000	0.000	0.000	0.000
	HH-type 6	2000	0.000	0.057	0.000	-0.057	0.000	0.000	0.000	0.000	0.000
		2005	0.000	0.226	0.000	0.226	0.000	0.000	0.000	0.000	0.000
	Income	2000	0.141	0.155	0.015	0.118	0.121	0.028	-0.044	0.000	0.099
		2005	0.267	0.311	0.054	0.404	-0.337	-0.057	-0.015	0.166	0.600
	Age	2000	0.142	0.320	-0.329	-4.908	-4.000	0.000	0.000	-2.000	0.000
Approach 4	HH-type 1	2000	0.000	0.045	0.000	-0.045	0.000	0.000	0.000	0.000	0.000
2000 vs 2004		2004	0.000	0.030	0.000	-0.030	0.000	0.000	0.000	0.000	0.000
$\Delta L_1 = 0.198$	HH-type 2	2000	0.000	0.035	0.000	-0.035	0.000	0.000	0.000	0.000	0.000
		2004	0.000	0.045	0.000	-0.045	0.000	0.000	0.000	0.000	0.000
	HH-type 3	2000	0.000	0.009	0.000	0.009	0.000	0.000	0.000	0.000	0.000
		2004	0.000	0.018	0.000	0.018	0.000	0.000	0.000	0.000	0.000
	HH-type 4	2000	0.000	0.130	0.000	0.130	0.000	0.000	0.000	0.000	0.000
		2004	0.000	0.082	0.000	0.082	0.000	0.000	0.000	0.000	0.000
	HH-type 5	2000	0.000	0.036	0.000	-0.036	0.000	0.000	0.000	0.000	0.000
		2004	0.000	0.052	0.000	-0.052	0.000	0.000	0.000	0.000	0.000
	HH-type 6	2000	0.000	0.024	0.000	-0.024	0.000	0.000	0.000	0.000	0.000
		2004	0.000	0.027	0.000	0.027	0.000	0.000	0.000	0.000	0.000
	Income	2000	0.126	0.133	0.000	0.099	0.110	0.087	0.000	0.036	-0.389
		2004	0.097	0.110	0.027	0.115	0.110	-0.050	0.047	0.014	-0.122
	Age	2000	0.065	0.123	-0.044	-0.594	-1.000	1.000	-1.000	-1.000	0.000
Approach 4	HH-type 1	2000	0.000	0.053	0.000	-0.053	0.000	0.000	0.000	0.000	0.000
2000 vs 2006		2006	0.000	0.047	0.000	-0.047	0.000	0.000	0.000	0.000	0.000
$\Delta L_1 = 0.167$	HH-type 2	2000	0.000	0.006	0.000	-0.006	0.000	0.000	0.000	0.000	0.000
		2006	0.000	0.062	0.000	-0.062	0.000	0.000	0.000	0.000	0.000
	HH-type 3	2000	0.000	0.003	0.000	0.003	0.000	0.000	0.000	0.000	0.000
		2006	0.000	0.012	0.000	0.012	0.000	0.000	0.000	0.000	0.000
	HH-type 4	2000	0.000	0.138	0.000	0.138	0.000	0.000	0.000	0.000	0.000
		2006	0.000	0.084	0.000	0.084	0.000	0.000	0.000	0.000	0.000
	HH-type 5	2000	0.000	0.032	0.000	-0.032	0.000	0.000	0.000	0.000	0.000
		2006	0.000	0.043	0.000	-0.043	0.000	0.000	0.000	0.000	0.000
	HH-type 6	2000	0.000	0.049	0.000	-0.049	0.000	0.000	0.000	0.000	0.000
		2006	0.000	0.056	0.000	0.056	0.000	0.000	0.000	0.000	0.000
	Income	2000	0.078	0.114	-0.005	0.124	0.028	0.081	-0.015	0.000	-0.148
		2006	0.039	0.148	-0.009	0.220	0.106	-0.011	0.000	-0.002	-0.059
	Age	2000	0.092	0.116	-0.011	-1.703	0.000	0.000	0.000	0.000	-1.000
Approach 4	HH-type 1	2000	0.000	0.060	0.000	-0.060	0.000	0.000	0.000	0.000	0.000
2000 vs 2007		2007	0.000	0.083	0.000	-0.083	0.000	0.000	0.000	0.000	0.000
$\Delta L_1 = 0.192$	HH-type 2	2000	0.000	0.006	0.000	-0.006	0.000	0.000	0.000	0.000	0.000
		2007	0.000	0.041	0.000	-0.041	0.000	0.000	0.000	0.000	0.000
	HH-type 3	2000	0.000	0.010	0.000	-0.010	0.000	0.000	0.000	0.000	0.000
		2007	0.000	0.021	0.000	0.021	0.000	0.000	0.000	0.000	0.000
	HH-type 4	2000	0.000	0.141	0.000	0.141	0.000	0.000	0.000	0.000	0.000
		2007	0.000	0.109	0.000	0.109	0.000	0.000	0.000	0.000	0.000
	HH-type 5	2000	0.000	0.033	0.000	-0.033	0.000	0.000	0.000	0.000	0.000
		2007	0.000	0.033	0.000	-0.033	0.000	0.000	0.000	0.000	0.000
	HH-type 6	2000	0.000	0.032	0.000	-0.032	0.000	0.000	0.000	0.000	0.000
		2007	0.000	0.027	0.000	0.027	0.000	0.000	0.000	0.000	0.000
	Income	2000	0.090	0.132	-0.023	0.126	0.028	0.003	-0.005	0.012	0.032
		2007	0.055	0.163	0.001	0.245	0.084	0.000	0.020	0.033	-0.050
	Age	2000	0.096	0.111	-0.007	-2.065	0.000	0.000	0.000	0.000	-1.000

Table continues

	Variables	Year	$L_1^{(j)}$		$I_1^{(j)}$		Between-group differences by quantiles				
			after	before	after	before	0	25	50	75	100
Approach 5	HH-type 1	2000	0.000	0.065	0.000	-0.065	0.000	0.000	0.000	0.000	0.000
2000 vs 2004		2004	0.000	0.064	0.000	-0.064	0.000	0.000	0.000	0.000	0.000
$\Delta L_1 = 0.125$	HH-type 2	2000	0.000	0.036	0.000	-0.036	0.000	0.000	0.000	0.000	0.000
		2004	0.000	0.038	0.000	-0.038	0.000	0.000	0.000	0.000	0.000
	HH-type 3	2000	0.000	0.018	0.000	-0.018	0.000	0.000	0.000	0.000	0.000
		2004	0.000	0.008	0.000	-0.008	0.000	0.000	0.000	0.000	0.000
	HH-type 4	2000	0.000	0.151	0.000	0.151	0.000	0.000	0.000	0.000	0.000
		2004	0.000	0.092	0.000	0.092	0.000	0.000	0.000	0.000	0.000
	HH-type 5	2000	0.000	0.019	0.000	-0.019	0.000	0.000	0.000	0.000	0.000
		2004	0.000	0.017	0.000	-0.017	0.000	0.000	0.000	0.000	0.000
	HH-type 6	2000	0.000	0.012	0.000	-0.012	0.000	0.000	0.000	0.000	0.000
		2004	0.000	0.035	0.000	0.035	0.000	0.000	0.000	0.000	0.000
	Income	2000	0.119	0.141	-0.010	0.159	0.110	0.013	-0.005	-0.055	-0.389
		2004	0.085	0.129	0.015	0.207	0.110	0.078	0.047	-0.016	-0.122
	Age	2000	0.051	0.100	-0.114	-0.071	-1.000	1.000	-1.000	0.000	0.000
Approach 5	HH-type 1	2000	0.000	0.057	0.000	-0.057	0.000	0.000	0.000	0.000	0.000
2000 vs 2006		2006	0.000	0.078	0.000	-0.078	0.000	0.000	0.000	0.000	0.000
$\Delta L_1 = 0.153$	HH-type 2	2000	0.000	0.022	0.000	-0.022	0.000	0.000	0.000	0.000	0.000
		2006	0.000	0.073	0.000	-0.073	0.000	0.000	0.000	0.000	0.000
	HH-type 3	2000	0.000	0.019	0.000	-0.019	0.000	0.000	0.000	0.000	0.000
		2006	0.000	0.013	0.000	-0.013	0.000	0.000	0.000	0.000	0.000
	HH-type 4	2000	0.000	0.146	0.000	0.146	0.000	0.000	0.000	0.000	0.000
		2006	0.000	0.069	0.000	0.069	0.000	0.000	0.000	0.000	0.000
	HH-type 5	2000	0.000	0.034	0.000	-0.034	0.000	0.000	0.000	0.000	0.000
		2006	0.000	0.003	0.000	0.003	0.000	0.000	0.000	0.000	0.000
	HH-type 6	2000	0.000	0.014	0.000	-0.014	0.000	0.000	0.000	0.000	0.000
		2006	0.000	0.092	0.000	0.092	0.000	0.000	0.000	0.000	0.000
	Income	2000	0.057	0.118	0.002	0.161	0.000	0.001	-0.005	0.002	-0.148
		2006	0.052	0.191	0.000	0.338	0.106	-0.046	0.000	-0.019	0.051
	Age	2000	0.061	0.107	0.028	-1.277	0.000	0.000	0.000	0.000	0.000
Approach 5	HH-type 1	2000	0.000	0.075	0.000	-0.075	0.000	0.000	0.000	0.000	0.000
2000 vs 2007		2007	0.000	0.123	0.000	-0.123	0.000	0.000	0.000	0.000	0.000
$\Delta L_1 = 0.146$	HH-type 2	2000	0.000	0.015	0.000	-0.015	0.000	0.000	0.000	0.000	0.000
		2007	0.000	0.069	0.000	-0.069	0.000	0.000	0.000	0.000	0.000
	HH-type 3	2000	0.000	0.035	0.000	-0.035	0.000	0.000	0.000	0.000	0.000
		2007	0.000	0.007	0.000	-0.007	0.000	0.000	0.000	0.000	0.000
	HH-type 4	2000	0.000	0.153	0.000	0.153	0.000	0.000	0.000	0.000	0.000
		2007	0.000	0.131	0.000	0.131	0.000	0.000	0.000	0.000	0.000
	HH-type 5	2000	0.000	0.028	0.000	-0.028	0.000	0.000	0.000	0.000	0.000
		2007	0.000	0.023	0.000	0.023	0.000	0.000	0.000	0.000	0.000
	HH-type 6	2000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
		2007	0.000	0.044	0.000	0.044	0.000	0.000	0.000	0.000	0.000
	Income	2000	0.080	0.160	-0.019	0.209	0.000	-0.061	-0.005	-0.032	0.032
		2007	0.091	0.224	0.009	0.412	0.084	0.004	0.006	0.000	-0.813
	Age	2000	0.056	0.101	0.005	-1.826	0.000	0.000	0.000	0.000	-1.000

Note. Own calculations. *Note.* HH-type 1: singles; HH-type 2: couples without children; HH-type 3: single parents; HH-type 4: couples with children aged 16 and younger; HH-type 5: couples with cohabiting children aged older than 16; HH-type 6: couples with younger and older children. Own calculations based on SOEP data, waves 2000 and 2004-2007. Data source: SOEP.

Table 10. Treatment effects for matched units

Approach	Waves	Group	Number of observations	Average treatment effect (s.e.)
1	2000 vs. 2004	treat	1,078	1.997**
		control	144	(0.814)
	2000 vs. 2005	treat	894	2.084**
		control	125	(0.954)
2	2000 vs. 2004	treat	321	-0.134
		control	81	(1.058)
	2000 vs. 2005	treat	310	3.168***
		control	72	(1.184)
3	2000 vs. 2004	treat	251	-0.320
		control	139	(0.788)
	2000 vs. 2005	treat	220	-1.477
		control	102	(0.793)
4	2000 vs. 2004	treat	195	0.137
		control	695	(0.600)
	2000 vs. 2006	treat	263	0.307
		control	604	(0.586)
	2000 vs. 2007	treat	266	-0.152
		control	504	(0.550)
5	2000 vs. 2004	treat	250	0.201
		control	732	(0.539)
	2000 vs. 2006	treat	316	0.230
		control	622	(0.556)
	2000 vs. 2007	treat	350	-0.669
		control	534	(0.508)

Note. *** p<0.01, ** p<0.05, * p<0.1. Standard errors in parentheses. Data source: SOEP.

Table 11. Random-effects tobit regressions (marginal effects): SAVE data

		Specification		
Approach 1		(1)	(2)	(3)
05-07	Treatment	1.806	1.403	2.109
		(1.515)	(1.498)	(1.559)
	Households		346 [88.44]	
05-08	Treatment	0.441	0.351	1.367
		(1.398)	(1.430)	(1.487)
	Households		318 [89.62]	
06-07	Treatment	0.886	0.303	0.389
		(0.904)	(0.899)	(0.952)
	Households		684 [82.89]	
06-08	Treatment	0.770	0.138	0.428
		(0.942)	(0.963)	(1.004)
	Households		598 [83.44]	
Approach 5				
05-07	Treatment	1.512	---	1.112
		(1.277)		(1.165)
	Households		143 [26.57]	
05-08	Treatment	2.806	---	2.706
		(1.714)		(1.776)
	Households		120 [27.50]	
06-07	Treatment	0.786	---	0.202
		(0.996)		(0.863)
	Households		250 [29.20]	
06-08	Treatment	2.146	---	1.766
		(1.390)		(1.860)
	Households		201 [31.84]	

Note. Savings refer to the previous year. (1): basic specification; (2): reduced set of controls; (3): full set of controls. *** p<0.01, ** p<0.05, * p<0.1. Standard errors in parentheses. Fraction of treated households in brackets. Calculations based on five multiply imputed datasets in accordance with Rubin (1987). Data source: SAVE.

Table 12. First-differences results: SAVE data

		Specification		
Approach 1		(1)	(2)	(3)
05-07	Treatment	1.238	0.589	0.655
		(1.829)	(1.898)	(1.977)
	Households		346 [88.44]	
05-08	Treatment	-0.135	-0.823	0.360
		(1.925)	(1.961)	(1.995)
	Households		318 [89.62]	
06-07	Treatment	0.390	0.438	0.350
		(1.143)	(1.138)	(1.164)
	Households		684 [82.89]	
06-08	Treatment	-0.201	-0.063	0.204
		(1.227)	(1.247)	(1.288)
	Households		598 [83.44]	
Approach 5				
05-07	Treatment	-0.450	---	-0.634
		(1.489)		(1.603)
	Households		143 [26.57]	
05-08	Treatment	0.625	---	1.203
		(1.829)		(2.030)
	Households		120 [27.50]	
06-07	Treatment	-0.809	---	-0.524
		(1.497)		(1.554)
	Households		250 [29.20]	
06-08	Treatment	-0.930	---	-0.435
		(1.679)		(1.772)
	Households		201 [31.84]	

Note. Savings refer to the previous year. (1): basic specification; (2): reduced set of controls; (3): full set of controls. *** p<0.01, ** p<0.05, * p<0.1. Standard errors in parentheses. Fraction of treated households in brackets. Calculations based on five multiply imputed datasets in accordance with Rubin (1987). Data source: SAVE.

Appendix

Proceedings: data preparation and sample selection

Household composition. We concentrate on single and couple households with and without children. Other household types are excluded from the analysis. In the SOEP, we combine information of the individual and the household questionnaire. A household unit is assessed as (non) eligible to Riester by the eligibility status of adult household members. Thereby, we neglect potential Riester eligibility of older but cohabiting children. In the later regression, we control for household type by distinguishing between the following types: HH-type 1: singles; HH-type 2: couples without children; HH-type 3: single parents; HH-type 4: couples with children aged 16 and younger; HH-type 5: couples with cohabiting children aged older than 16; HH-type 6: couples with younger and older children. The information provided in SAVE is different. Information is provided by only one household member, the household head. Household heads state individual information on themselves and the partner. In case of the SAVE data, in regressions we distinguish households composed by singles (HH-type 1), couples without cohabiting children (HH-type 2), single parent (HH-type 3), and couples with cohabiting children (HH-type 4).

Single households form the reference category in all regressions for Approaches 1-2 and 4-5 in both datasets. In Approach 3 (which considers couples with children), we use HH-type 4 (couples with children aged 16 and younger) as reference.

Identifying individuals eligible for Riester contracts. A crucial issue for our analysis is the eligibility for the Riester scheme. Eligibility is an individual attribute. However, we aggregate the Riester eligibility of the household heads and their partners on the household level and continue with all household observations with clear eligibility or non-eligibility status. Thus, a Riester household in our analysis is a household in which all adult members are eligible. In contrast, a household is classified as non-eligible if all adult household members are non-non. Riester eligibility is linked to the employment status of the individual. To begin with the SOEP, bluecollar and whitecollar workers, civil servants, trainees, apprentices, people in childcare leave and military/social service, people receiving early-retirement benefits, farmers, registered unemployed, as well as students with gross labor income higher than 400 euros are assigned a status as being eligible according to the legal requirements. In contrast, free-lancers and other self-employed (with dependent employees), pensioners, and students with low labor income are non-eligible. The same applies to non-working individuals and to recipients of widow pensions, orphan benefits, or social assistance if they do not derive an

eligibility status due to other reasons. If a person is eligible, we also assign this status to a spouse who lives in the same household. The proceeding is similar for the SAVE data, however with some minor exceptions. All free-lancers and self-employed (except farmers) are treated as non-eligible since we do not have information on the number of dependent employees. Moreover, training, retraining, apprenticeship, and academic studies form one answer category so that we cannot identify the single subgroups. We therefore exclude these households from the analysis.

In the regressions, we include employment dummies for self-employed, bluecollar worker, civil servant, unemployed, and other type. Whitecollar workers serve as reference category.

Construction of treatment and control groups. Treatment and control observations are grouped according to the scheme displayed in Table 2. For the different year comparisons, we build subsamples of households that appear in both periods and fulfill the necessary conditions in Table 2. For instance, in approach 3 we compare the savings ratio of married couple with two children vs. one child. For the 2000-2004 comparison, we use a subsample of all households surveyed with one or two in both periods.

Savings variables and control variables. The savings ratio is calculated by dividing the monthly regular savings amount by the net household income (SOEP). In SAVE, all income and savings variables refer to the previous year. We take the total amount of savings of the previous year, and divide it by 12 (to put it on a monthly basis) and by the average household net income. All income and savings variables are deflated according to the consumer price index provided by the Federal Statistical Office (2007).

Depending on the approach and the specification, we use different set of control variables. The age group variables refer to the age of the household head. We delete all household observations with a head younger than 20 or older than 55 in the reference 2002 (younger than 18 or older than 53 in 2000, etc.). The employment status also refers to the head of household. Whitecollar is the reference category. In addition, we control for bluecollar workers, civil servants, self-employed, unemployed, and others (which are people who do not belong to one of the other groups). A very small fraction in the SOEP declares more than one status (e.g., bluecollar but unemployed). We keep these very few individuals if their Riester eligibility status is unambiguous.

We also control for debt repayments in some specifications. The SOEP surveys repayments for consumer and housing loan. We use this information to construct the binary variables *credit* and *homeloan*. In SAVE, we proceed in a similar and use the control variables *credit* (for consumer credit repayments) and *homeloan* (comprising building loans and mortgages.

The weighted mean equivalent household income is calculated using the OECD-modified scale (http://www.oecd.org/LongAbstract/0,3425,en_2649_33933_35411112_1_1_1_1,00.html) which assigns a factor of one for the household head, a half for each additional adult, and 0.3 for each child. The resulting equivalent household income is weighted with the household weight provided with the SOEP and SAVE data (age and income), respectively. Finally, we delete observations with missing information in one of the relevant variables or with an income below the lowest or above the highest income percentile.

Table A1. Reliability of the SOEP savings variable

	Saving ratio Tobit	Saving (y/n) Probit
Household type 2	-0.470*	0.008
	(0.276)	(0.027)
Household type 3	-7.611***	-0.499***
	(0.342)	(0.031)
Household type 4	-4.829***	-0.259***
	(0.265)	(0.026)
Household type 5	-5.452***	-0.244***
	(0.334)	(0.035)
Household type 6	-7.426***	-0.490***
	(0.369)	(0.038)
University entrance qualification	0.666***	0.038*
	(0.218)	(0.023)
University degree	1.750***	0.083***
	(0.231)	(0.024)
Unemployed	-11.366***	-0.984***
	(0.367)	(0.030)
Blue collar	-1.818***	-0.179***
	(0.201)	(0.020)
Others	-4.737***	-0.462***
	(0.299)	(0.027)
Civil servant	-0.852***	0.084**
	(0.280)	(0.037)
Self-employed	-3.701***	-0.514***
	(0.370)	(0.034)
Income (defl.) in 1,000	3.492***	0.343***
	(0.087)	(0.011)
Age below 30 years	-1.412**	-0.092
	(0.638)	(0.056)
Age 30-39 years	-1.297**	-0.066
	(0.595)	(0.053)

Age 40-49 years	-2.523***	-0.174***
	(0.599)	(0.054)
Age 50 and above	-2.712***	-0.240***
	(0.612)	(0.055)
Credit	-5.622***	-0.407***
	(0.173)	(0.017)
Homeloan	-2.521***	-0.133***
	(0.179)	(0.019)
Dummy 2004	-1.476***	-0.113***
	(0.244)	(0.024)
Dummy 2005	-1.296***	-0.090***
	(0.246)	(0.025)
Dummy 2006	-1.535***	-0.132***
	(0.244)	(0.024)
Dummy 2007	-1.702***	-0.101***
	(0.243)	(0.025)
Constant	4.443***	0.139**
	(0.635)	(0.058)

Note. Number of observations: 31,198. Robust standard errors in parentheses. *** p<0.01,** p<0.05,* p<0.1. *Note.* HH-type 1: singles; HH-type 2: couples without children; HH-type 3: single parents; HH-type 4: couples with children aged 16 and younger; HH-type 5: couples with cohabiting children aged older than 16; HH-type 6: couples with younger and older children. Own calculations based on SOEP data, waves 2000 and 2004-2007.Data source: SOEP, waves 2000 and 2004-2007.

Table A2. Reliability of the SOEP savings variable: switchers (into and out of Riester contract)

	Saving (y/n)	Saving (y/n)	Saving (y/n)	Saving (y/n)	Saving (y/n)	Saving (y/n)
	Probit	Probit	Probit	Probit	Probit	Probit
	2004-2006	2004-2006	2004-2007	2004-2007	2006-2007	2006-2007
Switcher into Riester	0.117*	0.086	0.229***	0.168***	0.189***	0.187***
contract	(0.061)	(0.067)	(0.052)	(0.060)	(0.058)	(0.065)
Switcher out of Riester	-0.288***	-0.196*	-0.070	-0.042	-0.009	0.024
contract	(0.097)	(0.103)	(0.096)	(0.106)	(0.085)	(0.092)
Control variables	no	yes	no	yes	no	yes
Number of observations	4,900		4,585		5,510	

Note. Robust standard errors in parentheses. *** p<0.01,** p<0.05,* p<0.1. Data source: SOEP, waves 2004, 2006, 2007.

Table A3. SOEP data: Composition of treatment and control groups

Treat-ment approach	Year combi-nation	Group	Household type						White collar	Unemployed	Blue collar
			1	2	3	4	5	6			
1	2000	Treat	0.189	0.172	0.074	0.383	0.101	0.081	0.415	0.074	0.313
		Control	0.478	0.065	0.167	0.210	0.043	0.038	0.167	0.118	0.124
	2004	Treat	0.158	0.191	0.070	0.337	0.149	0.094	0.434	0.109	0.294
		Control	0.468	0.097	0.188	0.134	0.065	0.048	0.000	0.000	0.000
	2000	Treat	0.186	0.174	0.073	0.385	0.098	0.083	0.414	0.071	0.318
		Control	0.448	0.086	0.190	0.190	0.049	0.037	0.209	0.147	0.110
	2005	Treat	0.153	0.196	0.073	0.326	0.156	0.096	0.426	0.113	0.299
		Control	0.485	0.104	0.209	0.098	0.080	0.025	0.000	0.000	0.000
2	2000	Treat	0.163	0.083	0.115	0.470	0.074	0.094	0.286	0.137	0.409
		Control	0.425	0.057	0.217	0.198	0.057	0.047	0.142	0.170	0.151
	2004	Treat	0.147	0.091	0.112	0.382	0.139	0.129	0.318	0.195	0.385
		Control	0.434	0.075	0.264	0.104	0.057	0.066	0.000	0.000	0.000
	2000	Treat	0.167	0.081	0.112	0.463	0.077	0.099	0.295	0.126	0.411
		Control	0.420	0.080	0.260	0.150	0.040	0.050	0.180	0.230	0.130
	2005	Treat	0.146	0.100	0.119	0.353	0.152	0.131	0.318	0.193	0.391
		Control	0.460	0.110	0.270	0.070	0.050	0.040	0.000	0.000	0.000
3	2000	Treat	0.000	0.000	0.000	0.834	0.046	0.120	0.267	0.086	0.469
		Control	0.000	0.000	0.000	0.641	0.266	0.094	0.223	0.117	0.508
	2004	Treat	0.000	0.000	0.000	0.629	0.162	0.208	0.311	0.104	0.473
		Control	0.000	0.000	0.000	0.504	0.496	0.000	0.234	0.188	0.465
	2000	Treat	0.000	0.000	0.000	0.863	0.036	0.100	0.267	0.075	0.474
		Control	0.000	0.000	0.000	0.604	0.238	0.157	0.272	0.111	0.494
	2005	Treat	0.000	0.000	0.000	0.594	0.179	0.226	0.329	0.090	0.451
		Control	0.000	0.000	0.000	0.413	0.587	0.000	0.264	0.132	0.515
4	2000	Treat	0.119	0.053	0.115	0.593	0.045	0.074	0.358	0.119	0.370
		Control	0.164	0.088	0.106	0.463	0.081	0.098	0.285	0.145	0.427
	2004	Treat	0.115	0.053	0.123	0.461	0.095	0.152	0.407	0.173	0.366
		Control	0.145	0.098	0.106	0.379	0.147	0.125	0.305	0.195	0.389
	2000	Treat	0.116	0.079	0.113	0.585	0.055	0.052	0.351	0.101	0.372
		Control	0.169	0.085	0.110	0.447	0.087	0.101	0.291	0.141	0.425
	2006	Treat	0.101	0.067	0.125	0.402	0.134	0.171	0.409	0.116	0.378
		Control	0.148	0.129	0.113	0.318	0.177	0.115	0.303	0.211	0.391
	2000	Treat	0.122	0.081	0.096	0.588	0.052	0.061	0.357	0.122	0.359
		Control	0.182	0.087	0.106	0.448	0.085	0.093	0.296	0.148	0.425
	2007	Treat	0.087	0.087	0.119	0.388	0.157	0.162	0.435	0.110	0.371
		Control	0.170	0.128	0.098	0.280	0.189	0.135	0.299	0.202	0.416
5	2000	Treat	0.104	0.054	0.092	0.604	0.060	0.085	0.332	0.120	0.389
		Control	0.169	0.090	0.110	0.454	0.079	0.097	0.286	0.146	0.427
	2004	Treat	0.089	0.060	0.101	0.465	0.127	0.158	0.373	0.171	0.370
		Control	0.153	0.099	0.109	0.373	0.144	0.123	0.305	0.195	0.391
	2000	Treat	0.115	0.067	0.096	0.584	0.055	0.082	0.310	0.108	0.413
		Control	0.172	0.089	0.115	0.438	0.089	0.096	0.300	0.143	0.415
	2006	Treat	0.079	0.063	0.106	0.387	0.171	0.195	0.377	0.115	0.394
		Control	0.158	0.135	0.118	0.318	0.167	0.103	0.305	0.218	0.388
	2000	Treat	0.113	0.074	0.081	0.585	0.062	0.085	0.326	0.117	0.398
		Control	0.188	0.089	0.116	0.432	0.089	0.086	0.293	0.156	0.420
	2007	Treat	0.064	0.072	0.100	0.391	0.200	0.172	0.398	0.106	0.394
		Control	0.187	0.141	0.107	0.260	0.177	0.128	0.295	0.208	0.407

Table continues

Table A3 continued

Treat-ment approach	Year combi-nation	Group	Other empl.	Civil serva nt	Self empl oyed	Income in €1,000	Age in years				Credit	Home-loan
							<30	30-39	40-49	50+		
1	2000	Treat	0.093	0.069	0.039	2.567	0.117	0.394	0.344	0.114	0.347	0.298
		Control	0.323	0.022	0.263	2.056	0.108	0.296	0.333	0.226	0.280	0.194
	2004	Treat	0.065	0.067	0.041	2.786	0.039	0.325	0.390	0.230	0.358	0.341
		Control	0.629	0.000	0.371	2.015	0.081	0.210	0.382	0.323	0.280	0.210
	2000	Treat	0.092	0.069	0.041	2.574	0.114	0.396	0.347	0.111	0.350	0.301
		Control	0.294	0.018	0.233	2.029	0.080	0.270	0.368	0.258	0.313	0.202
	2005	Treat	0.070	0.067	0.041	2.795	0.029	0.296	0.403	0.264	0.270	0.349
		Control	0.626	0.000	0.374	1.985	0.037	0.202	0.301	0.454	0.172	0.190
2	2000	Treat	0.133	0.012	0.030	2.007	0.135	0.413	0.334	0.084	0.351	0.223
		Control	0.462	0.009	0.094	1.451	0.142	0.245	0.321	0.245	0.264	0.160
	2004	Treat	0.075	0.012	0.030	2.109	0.054	0.334	0.410	0.187	0.373	0.259
		Control	0.877	0.000	0.123	1.316	0.104	0.179	0.349	0.358	0.189	0.160
	2000	Treat	0.125	0.018	0.030	2.019	0.130	0.412	0.342	0.082	0.363	0.223
		Control	0.380	0.010	0.090	1.428	0.060	0.250	0.410	0.260	0.300	0.160
	2005	Treat	0.077	0.017	0.033	2.096	0.037	0.300	0.428	0.224	0.280	0.272
		Control	0.830	0.000	0.170	1.288	0.040	0.170	0.290	0.500	0.170	0.110
3	2000	Treat	0.134	0.018	0.032	2.367	0.074	0.569	0.293	0.026	0.371	0.351
		Control	0.102	0.016	0.039	2.208	0.063	0.324	0.457	0.141	0.379	0.246
	2004	Treat	0.066	0.018	0.036	2.591	0.008	0.427	0.471	0.088	0.405	0.401
		Control	0.070	0.020	0.031	2.197	0.012	0.246	0.434	0.285	0.379	0.277
	2000	Treat	0.137	0.028	0.024	2.384	0.088	0.562	0.282	0.024	0.357	0.353
		Control	0.068	0.013	0.047	2.266	0.038	0.332	0.477	0.145	0.409	0.268
	2005	Treat	0.073	0.028	0.038	2.615	0.009	0.393	0.481	0.115	0.297	0.423
		Control	0.055	0.009	0.034	2.210	0.000	0.170	0.438	0.387	0.336	0.311
4	2000	Treat	0.144	0.008	0.012	2.114	0.115	0.481	0.288	0.078	0.399	0.309
		Control	0.106	0.014	0.031	2.015	0.132	0.403	0.345	0.087	0.350	0.216
	2004	Treat	0.049	0.008	0.012	2.222	0.049	0.342	0.444	0.148	0.469	0.309
		Control	0.080	0.012	0.033	2.107	0.049	0.332	0.407	0.196	0.361	0.258
	2000	Treat	0.137	0.018	0.027	2.139	0.159	0.451	0.280	0.061	0.430	0.305
		Control	0.110	0.017	0.024	2.015	0.125	0.403	0.342	0.094	0.364	0.208
	2006	Treat	0.079	0.015	0.027	2.257	0.027	0.308	0.491	0.165	0.320	0.369
		Control	0.073	0.014	0.034	2.037	0.025	0.270	0.418	0.278	0.265	0.259
	2000	Treat	0.130	0.009	0.029	2.119	0.171	0.470	0.272	0.046	0.386	0.284
		Control	0.100	0.017	0.021	1.993	0.126	0.410	0.336	0.094	0.374	0.210
	2007	Treat	0.061	0.009	0.035	2.279	0.020	0.287	0.472	0.209	0.307	0.359
		Control	0.058	0.017	0.040	2.034	0.015	0.241	0.426	0.311	0.265	0.267
5	2000	Treat	0.139	0.009	0.025	2.161	0.114	0.462	0.313	0.079	0.396	0.316
		Control	0.105	0.014	0.029	2.002	0.133	0.403	0.342	0.087	0.349	0.210
	2004	Treat	0.063	0.009	0.028	2.295	0.044	0.307	0.472	0.161	0.465	0.329
		Control	0.079	0.012	0.031	2.088	0.050	0.339	0.399	0.197	0.358	0.252
	2000	Treat	0.123	0.022	0.029	2.161	0.139	0.457	0.300	0.063	0.433	0.291
		Control	0.112	0.016	0.023	2.000	0.128	0.398	0.341	0.096	0.359	0.208
	2006	Treat	0.084	0.019	0.034	2.335	0.029	0.291	0.493	0.180	0.320	0.373
		Control	0.071	0.013	0.032	1.997	0.024	0.274	0.411	0.281	0.262	0.250
	2000	Treat	0.123	0.013	0.028	2.169	0.157	0.460	0.287	0.057	0.394	0.296
		Control	0.104	0.015	0.021	1.960	0.121	0.399	0.345	0.101	0.365	0.202
	2007	Treat	0.070	0.015	0.036	2.374	0.021	0.283	0.457	0.230	0.323	0.379
		Control	0.071	0.014	0.037	1.962	0.014	0.233	0.424	0.323	0.254	0.252

Note. HH-type 1: singles; HH-type 2: couples without children; HH-type 3: single parents; HH-type 4: couples with children aged 16 and younger; HH-type 5: couples with cohabiting children aged older than 16; HH-type 6: couples with younger and older children. Own calculations based on SOEP data, waves 2000 and 2004-2007.

Table A4. SAVE data: Composition of treatment and control groups

Treat-ment approach	Year combi-nation	Group	Household type				White collar	Unemp loyed	Blue collar	Other s	Civil servant	Self employ ed
			1	2	3	4						
1	2005	Treat	0.136	0.160	0.133	0.572	0.318	0.293	0.157	0.267	0.026	0.004
		Control	0.292	0.333	0.135	0.240	0.057	0.125	0.005	0.740	0.000	0.073
	2007	Treat	0.142	0.179	0.129	0.549	0.337	0.306	0.196	0.214	0.031	0.003
		Control	0.370	0.412	0.161	0.057	0.000	0.000	0.000	0.953	0.000	0.047
	2005	Treat	0.120	0.165	0.120	0.595	0.317	0.279	0.167	0.285	0.025	0.007
		Control	0.355	0.444	0.083	0.118	0.118	0.171	0.000	0.681	0.000	0.059
	2008	Treat	0.120	0.190	0.113	0.577	0.333	0.254	0.177	0.254	0.037	0.000
		Control	0.414	0.414	0.112	0.059	0.000	0.000	0.000	0.941	0.000	0.059
	2006	Treat	0.128	0.217	0.125	0.529	0.341	0.227	0.131	0.329	0.030	0.007
		Control	0.337	0.266	0.188	0.209	0.016	0.071	0.021	0.860	0.019	0.017
	2007	Treat	0.125	0.233	0.124	0.519	0.349	0.240	0.150	0.287	0.030	0.002
		Control	0.354	0.292	0.197	0.157	0.000	0.000	0.000	0.991	0.000	0.009
	2006	Treat	0.108	0.214	0.121	0.556	0.325	0.210	0.145	0.344	0.028	0.010
		Control	0.359	0.333	0.148	0.160	0.038	0.070	0.054	0.848	0.000	0.010
	2008	Treat	0.098	0.248	0.115	0.538	0.350	0.208	0.158	0.294	0.037	0.002
		Control	0.369	0.363	0.168	0.100	0.000	0.000	0.000	0.980	0.000	0.020
5	2005	Treat	0.105	0.105	0.131	0.660	0.183	0.262	0.267	0.314	0.000	0.000
		Control	0.229	0.134	0.219	0.418	0.216	0.540	0.214	0.145	0.000	0.000
	2007	Treat	0.105	0.131	0.105	0.660	0.372	0.340	0.215	0.256	0.000	0.000
		Control	0.229	0.134	0.219	0.418	0.269	0.487	0.214	0.155	0.000	0.000
	2005	Treat	0.153	0.031	0.239	0.577	0.362	0.245	0.215	0.270	0.000	0.000
		Control	0.217	0.137	0.162	0.483	0.199	0.579	0.178	0.188	0.000	0.000
	2008	Treat	0.123	0.123	0.215	0.540	0.374	0.276	0.202	0.209	0.000	0.000
		Control	0.217	0.172	0.172	0.439	0.135	0.492	0.227	0.238	0.000	0.000
	2006	Treat	0.097	0.139	0.153	0.611	0.320	0.345	0.169	0.261	0.000	0.000
		Control	0.225	0.135	0.185	0.455	0.256	0.415	0.189	0.247	0.007	0.011
	2007	Treat	0.111	0.153	0.153	0.583	0.355	0.320	0.186	0.247	0.014	0.000
		Control	0.208	0.140	0.180	0.472	0.273	0.409	0.183	0.235	0.006	0.002
	2006	Treat	0.130	0.035	0.222	0.614	0.370	0.332	0.145	0.275	0.000	0.000
		Control	0.187	0.194	0.152	0.466	0.179	0.408	0.253	0.251	0.010	0.015
	2008	Treat	0.114	0.098	0.206	0.582	0.389	0.253	0.209	0.231	0.000	0.000
		Control	0.173	0.231	0.152	0.444	0.177	0.428	0.234	0.231	0.015	0.000

Table continues

Table A4 continued

Approach	Year combi-nation	Group	Income in €1,000	Age in years				Credit	Homeloan
				<30	30-39	40-49	50+		
1	2005	Treat	2.007	0.093	0.293	0.367	0.247	0.171	0.252
		Control	1.574	0.010	0.104	0.156	0.729	0.026	0.141
	2007	Treat	1.946	0.058	0.243	0.397	0.302	0.155	0.241
		Control	1.551	0.005	0.109	0.104	0.781	0.078	0.188
	2005	Treat	2.062	0.095	0.268	0.369	0.267	0.185	0.269
		Control	1.556	0.030	0.142	0.089	0.740	0.059	0.071
	2008	Treat	1.874	0.046	0.205	0.398	0.352	0.139	0.270
		Control	1.603	0.000	0.083	0.118	0.799	0.124	0.089
	2006	Treat	2.014	0.079	0.250	0.382	0.289	0.146	0.303
		Control	1.742	0.019	0.078	0.242	0.661	0.079	0.164
	2007	Treat	2.077	0.059	0.234	0.368	0.339	0.174	0.270
		Control	1.696	0.019	0.078	0.207	0.696	0.102	0.169
	2006	Treat	2.048	0.080	0.227	0.382	0.311	0.149	0.308
		Control	1.746	0.010	0.108	0.200	0.683	0.074	0.150
	2008	Treat	2.004	0.040	0.209	0.366	0.385	0.157	0.287
		Control	1.621	0.010	0.058	0.180	0.753	0.152	0.148
5	2005	Treat	1.553	0.052	0.492	0.298	0.157	0.110	0.199
		Control	1.252	0.115	0.193	0.378	0.315	0.130	0.099
	2007	Treat	1.610	0.026	0.288	0.450	0.236	0.115	0.241
		Control	1.154	0.067	0.191	0.389	0.353	0.097	0.122
	2005	Treat	1.426	0.092	0.448	0.338	0.123	0.104	0.135
		Control	1.254	0.137	0.153	0.366	0.343	0.146	0.140
	2008	Treat	1.504	0.031	0.307	0.417	0.245	0.166	0.184
		Control	1.133	0.057	0.160	0.371	0.412	0.094	0.124
	2006	Treat	1.540	0.097	0.350	0.289	0.264	0.128	0.278
		Control	1.198	0.073	0.235	0.400	0.292	0.146	0.167
	2007	Treat	1.543	0.069	0.308	0.330	0.292	0.133	0.225
		Control	1.232	0.051	0.229	0.400	0.320	0.131	0.146
	2006	Treat	1.459	0.063	0.456	0.307	0.174	0.127	0.241
		Control	1.201	0.087	0.161	0.389	0.363	0.123	0.181
	2008	Treat	1.508	0.032	0.396	0.329	0.244	0.136	0.196
		Control	1.185	0.029	0.181	0.343	0.447	0.110	0.151

Note. HH-type 1: singles; HH-type 2: couples without children; HH-type 3: single parents; HH-type 4: couples with children. Employment states and age refer to the head of household. Own calculations. Data source: SAVE, 2005-2008.